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TRANSACTIONS

OF THE

ROYAL ACADEMY OF MEDICINE IN IRELAND.

TRANSACTIONS

OF THE

Royal Academy of Medicine

IN IRELAND.

VOL. VIII.

BOSTON MEDICAL LIBRARY ASSN.
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DUBLIN:

FANNIN AND COMPANY, GRAFTON-STREET.

LONDON: BAILLIÈRE, TINDALL, & COX.

EDINBURGH: MACLACHLAN & STEWART.

1890.

1037

CATALOGUED,
M. J.
5. 2. 1891.

DUBLIN: PRINTED BY JOHN FALCONER, 53 UPPER SACKVILLE-STREET.

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1884 STRAHAN, MICHAEL, L.R.C.S., Medical Officer, No. 2 North City Dispensary District, 38 Rutland-square, Dublin.

1886 TATE, D. D., junr., L.R.C.P., Assistant Medical Officer Mountjoy Convict Prison, Dublin.

1887 THOMPSON, W. H., M.D., F.R.C.S., Eng., Demonstrator of Anatomy, Trinity College, 20 Lower Baggot street. [1]

1889 TWEEDY, ERNEST HASTINGS, L.R.C.P., L.R.C.S., House Surgeon, Steevens' Hospital, Dublin.

† WHITE, WM. DUDLEY, L.R.C.S., Medical Officer, No. 3 Dispensary District, North Dublin Union, 51 Rutland-square, Dublin.

1887 WYNNE, GEO. NESBITT, M.D., M.Ch., 77 Aungier-street, Dublin.

STUDENT ASSOCIATES.

BISHOP, J., Dublin.

BLANEY, A. J., Dublin.

BOURKE, JOHN J., Medical School, Cecilia-street, Dublin.

BRABAZON, E. C., Dublin.

BRANNAN, F., Dublin.

CAHILL, T., Dublin.

CREIGHTON, R., Dublin.

FITZGERALD, J. J., Dublin.

FAGAN, P. J., Dublin.

GREEN, FRED. J., Dublin.

JACOB, H. W., Ballybrack.

LEDWITH, MATHEW, Dublin.

MCFARLAND, A. J., Dublin.

M'GEE, T. H., Medical School, Cecilia-street, Dublin.

O'GORMAN, W., Dublin.

ROBINSON, OLIVER, Meath Hospital, Dublin.

TATE, G. W., Dublin.

WALTER, DARBY, Dublin.

WATTS, VINCENT LEONARD, Dublin.

WILSON, S. W., Dublin.

RULES.

1. The name shall be, "ROYAL ACADEMY OF MEDICINE IN IRELAND." (1887.)

Constitution.

2. The Academy shall consist of Fellows, Honorary Fellows, Members, and Student Associates.

Management.

3. The affairs shall be managed by a Council, consisting of the President, the six Presidents of Sections, the General Secretary and Treasurer, the Secretary for Foreign Correspondence, six Secretaries of Sections, and eight Councillors, being two representatives from the Medical, Surgical, Obstetrical, and Pathological Sectional Councils respectively.

Meetings.

4. The Meetings shall be General and Ordinary.

Publication of "Transactions."

5. The "Transactions" shall be published by the Council, subject to the provisions hereinafter contained.

Original Fellows and Members.

6. All the Members of the present Societies (Medical, Surgical, Obstetrical, and Pathological) shall be Original Fellows or Members, without entrance fee, on payment of the annual subscription on or before 31st December, 1882.[a]

Fellows.

7. Fellows of the King and Queen's College of Physicians in Ireland, and of the Royal College of Surgeons in Ireland, shall be admitted, without ballot, on payment of the entrance fee and the subscription for the current year. All others, being Registered Medical Practitioners not directly or indirectly engaged in the sale of drugs, shall be proposed by two Fellows, and elected by ballot by the Council.

8. Candidates shall be proposed at one Meeting of the Council, and balloted for at the next—one black bean in four to reject.

8A. That all Rules referring to the admission of Fellows, Members, and Student Associates shall be interpreted as referring to Ladies as well as Gentlemen.

Privileges of Fellows.

9. Fellows only shall be eligible for office in the Academy. They shall have the privilege of attending all Meetings of the Academy, of making Communications, and of voting and speaking at such meetings. They shall also receive a copy of the "Transactions."

[a] Those who have paid a Life Subscription to any of the above Societies will be admitted to the privilege of Fellows on payment of Member's subscription.

10. These privileges shall not be exercised by any Fellow in arrear with his subscription.

Honorary Fellows.

11. Honorary Fellows, limited in number to 25, may be nominated by the Council, and elected, on motion at a General Meeting of the Academy by a majority of at least two-thirds of those present and voting.

Members.

12. Any Registered Medical Practitioner may be elected as a Member, the election to be conducted in the same manner as that of Fellows.

Privileges of Members.

13. Members shall have the privilege of attending the Ordinary Meetings of the Academy, of making Communications, and of taking part in debate. They can purchase the "Transactions" at cost price.

Student Associates.

14. Registered Medical Students, of the third or subsequent years, may be elected as Student Associates in the same manner as the Members.

15. Student Associates shall have the privilege of attending the Ordinary Meetings of the Academy.

Annual Subscription.

16. Fellows shall pay £2 2s., and Members £1 1s. Student Associates shall pay 5s. The Subscription shall become due on the 1st of October in each year, and if the Subscription be not paid on or before the first Meeting in February, the defaulter shall cease to belong to the Academy, unless the delay shall be accounted for to the satisfaction of the Council. No Fellow shall vote at the Annual General Meeting who has not paid his subscription for the year. Medical Officers of the Army and Navy, and Registered Medical Practitioners not residing within 15 miles of Dublin, are eligible as Fellows of the Academy on payment of the entrance fee, and an annual Subscription of £1 1s.

Entrance Fee.

17. After admission of Original Fellows, all Fellows shall pay an entrance fee of £1 1s.

Council.

18. The Council shall meet on the first Wednesday in the month throughout the Session, or oftener should they see occasion—five to form a quorum.

19. Notice of all Extraordinary Meetings shall be transmitted by the Secretary to every Member of the Council. The President or any five Members of Council may call an Extraordinary Meeting of the Council. The Council shall determine questions by vote, or by division if so demanded, the President having a casting vote only. Any regulation of the Council shall have the force of a law, until submitted to the next General Meeting. The Council shall have the power of filling up any vacancies which may occur in the list of Officers of the Academy, except that of President, before the Annual General Meeting. If a vacancy in the office of President should occur, the General Council shall summon a Special General Meeting of the Academy to fill such vacancy. (1888.)

Sectional Councils.

20. There shall be six Sectional Councils elected by the Annual General Meeting in October, termed respectively—the Medical, the Surgical, the Obstetrical, and the Pathological, the State Medicine, and the Anatomical and Physiological Councils.

21. No Fellow shall be eligible as a candidate for election on more than two Sectional Councils, but no Fellow shall be eligible as a candidate for election on both the Medical and Surgical Sectional Councils. (1888.)

22. Each Sectional Council shall consist of the President of the Section and ten Members, one of whom shall act as Secretary to the Section ; except the State Medicine and Anatomical and Physiological Councils, which shall each consist of a President and six Members. (1888).

Meetings of Sectional Councils.

23. Each Sectional Council shall meet on a fixed day at least one week before the Ordinary Meeting of their Section, three to form a quorum.

Powers.

24. Each Sectional Council shall have the power of making any such arrangements as it thinks necessary to carry on the work of the Ordinary Meetings which are under its charge, provided that such arrangements do not interfere with the general laws of the Academy ; and any Rules laid down by such Council shall have the force of laws at the Ordinary Meetings under its charge, until submitted to the General Council.

25. Each Sectional Council shall have the power of filling up any vacancies that may occur among its Members until the Annual General Meeting.

Committee of Reference.

26. The Council shall appoint a Committee of Reference, to report upon morbid growths and other specimens exhibited before the Academy ; of this Committee the Exhibitor shall, for the occasion, be a Member.

Officers.

27. A President, to be elected by the Annual General Meeting in October, and to hold office for three years.

28. The Presidents of the Colleges of Physicians and Surgeons for the time being shall be the Presidents of the Medical and Surgical Sections. The Presidents of the other Sections shall be elected by the Fellows at the Annual General Meeting, and shall hold office for two years. (1888.)

29. One General Secretary and Treasurer to be elected at the Annual General Meeting.

30. It is expedient that a fixed salary (of one hundred guineas) shall be paid yearly to the General Secretary in consideration of the fact that the editing of the "Transactions" is part of his duties.

31. One Honorary Secretary for Foreign Correspondence to be elected at the Annual General Meeting. (1888.)

32. The Councillors for each Section to be elected at the Annual General Meeting. Each Sectional Council shall elect two Members to act on the General Council, except in the case of the Sections of State Medicine and Anatomy and Physiology. (1888.)

33. Two Members in each Sectional Council shall retire annually, and be ineligible for re-election for one year.

34. Six Secretaries, one for each Section, to be appointed by the Sectional Councils.

35. At all elections after the year 1882, any Fellow desirous of nominating a candidate for election on any of the Sectional Councils shall, at least one fortnight before the Annual General Meeting, forward an application to the General Secretary to enter the name of such Fellow on the list of candidates for the office of Councillor of that Section, provided that the Fellow so nominated shall have consented to act. (1886.)

36. That all elections shall be by ballot.

Duties of Officers.

37. *The President* shall preside at the Annual and Special General Meetings and at General Council Meetings. In the absence of the President, the Chairman shall be appointed by the meeting. (1888.)

38. *The Presidents of Sections* shall preside at the Ordinary Meetings of the Academy, and shall also preside at the Sectional Council Meetings. In the absence of the President, the Chairman shall be appointed by the Meeting. (1888).

39. *The General Secretary* shall attend all General Meetings of the Academy and General Council. He shall take minutes of such meetings, to be read at the following meeting.

40. He shall receive and have charge of all papers intended for publication in the "Transactions" of the Academy, after they have been handed over to him by the Secretaries of the several Sections.

41. He shall, on receiving notice from the Secretary of a Section, send out to all the Members notices of the title or titles of the paper or papers for the next Ordinary Meeting, with the name or names of the authors, and, so far as possible, of the subjects for Exhibition, with the names of the Exhibitors.

42. He shall arrange for the Exhibition of specimens and the reading of papers, which are forwarded to the Academy by those who are absent, or are not members.

43. The General Secretary and Treasurer shall receive all moneys, and lodge the same in bank to the account of the Academy, and all cheques shall be signed by the Treasurer and one other Councillor.

44. The Accounts shall be audited by two Fellows, not Members of Council, to be appointed by the President at some meeting previous to the Annual Meeting.

Duties of Secretaries of Sections.

45. To attend the Meetings of the Council of the Section and the Ordinary Meetings of the Academy, under the management of said Council, and to take minutes at such meetings, to be read at the next following meeting of that Section.

46. To keep such papers as the Sectional Councils deem worthy of publication, for the purpose of handing them over to the General Secretary.

47. To inform the Secretary of the Committee of Reference of any specimens referred to that Committee, and to transfer the specimens to that Secretary.

48. To give notice to the General Secretary, one week previously to the meeting, of the titles of papers for the evening, the names of the authors, and, so far as possible, the objects for Exhibition, with the names of Exhibitors, so that the General Secretary may inform the Members.

Meetings.

49. The Annual General Meeting to take place on the last Friday in October, for the election of Officers and Members of Council, and for the general business of the Academy.

50. Due notice of the meeting shall be given by the Secretary to all members one fortnight previously, and the names of Candidates proposed for office must be in the hands of the Secretary one week before the meeting.

51. No motion involving a change of these Rules shall be brought before this meeting except one week's notice thereof shall have been given by the Secretary to each Member.

52. The President may—and shall forthwith, on receiving a requisition signed by seven Fellows, at any time—on giving one week's notice, summon a Special General Meeting, for the consideration of particular business, the nature of which must be specified in the letter of summons convening the meeting, and at such meeting no other business can be transacted. In the event of the President being unable, from any cause, or declining, to summon a Special General Meeting of the Academy, it shall be in the power of the General Council to summon such meeting. (1888.)

Ordinary Meetings.

53. The communications to be submitted to the Ordinary Meetings shall be grouped under the following heads :—Medicine, Surgery, Pathology, Obstetrics State Medicine, and Anatomy and Physiology ; and the conduct of such meetings shall be in the hands of the several Sectional Councils, each Sectional Council to have the management of the Ordinary Meeting in rotation, as arranged by the General Council. (1888.)

54. The Ordinary Meetings shall be held on every Friday evening, from the first Friday in November until the last Friday in May, inclusive, at eight o'clock, except during the Christmas and Easter recesses.

55. All Fellows, Members, and Student Associates attending the meetings, shall write their names in the attendance book.

56. Any Fellow or Member may introduce two Visitors by cards obtained from the Sectional Secretaries.

57. Officers of the Army or Navy Medical Departments shall, on presenting their cards, be admitted to the Ordinary Meetings of the Academy.

58. No communication shall exceed twenty minutes in its delivery, nor any speech thereon ten minutes, except by permission of the Chairman. No one shall speak twice upon the same communication, except the author, who has the right of reply.

Ordinary Meetings.—Order of Business.

59. (1.) Chair to be taken at 8 30 p.m.

(2.) Chairman to read list of specimens, &c., exhibited by card, together with the names of the Exhibitors.

(3.) No Pathological Specimen shall be exhibited at any Section other than the Pathological and Obstetrical, except by card. This Exhibition shall not exclude any subsequent communication regarding it at the Pathological Section.

(4.) There shall be no Exhibition of Specimens by card in the Obstetrical or Pathological Sections.

(5.) Any member shall have liberty to exhibit any recent specimen at any of the meetings of the Obstetrical Section, provided it illustrates any question in gynæcology.

(6.) At the meetings of the Obstetrical Section recent specimens may be exhibited, and the President shall invite discussion thereon, provided that such exhibition of specimens or discussion, if any, thereon, must terminate at 9 o'clock, p.m., but that, if necessary, they may be resumed after the papers for the evening have been read and discussed.

(7.) Chairman to ask if any member has any observations to make or motion to propose relative to any living specimen on the List of Exhibition.

(8.) Chairman to call upon the author of the first paper on the list to read his paper.

(9.) Chairman to call upon members to discuss the paper, or, at his discretion, to take any other paper or papers on the list relating to the subject, and have the discussion subsequently on all such papers collectively.

(10.) When the last paper has been discussed, the Chairman to ask if any member desires to speak upon any of the specimens exhibited by card.

(11.) After the discussion upon any specimen, the Exhibitor has the right of reply.

Regulations regarding the Exhibition of Specimens by Card.

60. (1.) Any member may exhibit by card at any Ordinary Meeting, except at the meeting of the Pathological and Obstetrical Sections. At the meetings of the Pathological all specimens must be presented and described *viva voce*, and debate may be invited thereon.

(2.) Notice shall, if possible, be given to the General Secretary, or the Secretary of the Section, on or before the previous Ordinary Meeting.

(3.) Specimens must be in the room at 7 45 on the night of Exhibition.

(4.) Specimens for Exhibition by card shall be open for inspection at 8 p.m.

(5.) A card, containing all particulars for publication, shall be placed with the Specimen. Cards for this purpose are to be obtained from the Secretary.

(6.) The Exhibitor should be present, and he shall furnish further details if asked for.

(7.) Every Exhibitor shall submit the Specimen or Specimens on view to the Committee of Reference, if the meeting so decide.

Exhibition of Pathological Specimens.

61. No lengthened reference to treatment shall be allowed upon any Specimen, except by the express permission of the Chairman. Whenever it has been agreed that a Specimen exhibited at a Sectional Meeting of the Royal Academy of Medicine in Ireland shall be sent to the Reference Committee to report thereon as to its nature, the Exhibitor is to retain the custody of the specimen until he shall be summoned to a meeting of said Committee to be convened by its Secretary, on an early day, when he will attend and submit it for examination. (1889).

By-laws concerning "Transactions."

62. The "Transactions" shall consist of such Communications made to the Academy by or through Fellows or Members as may be deemed by the General Council suitable for publication; also, of discussions of importance or interest arising out of such Communications.

63. All Communications accepted by the Academy become the property of the Academy, but authors may also print their Communications in any publication in addition to the "Transactions." Papers shall be handed to the Secretary of the Section immediately after they have been read.

64. The "Transactions" for the year shall be presented to all Fellows of the Academy who have paid their Annual Subscriptions.

65. The "Transactions" may be purchased by Members at cost price.

66. The Publication Committee of each Section shall meet not later than the Tuesday after each meeting of the Section, for the purpose of abstracting the proceedings—the abstract to be placed in the printer's hands on same evening, and forwarded to the editors of medical journals with the least possible delay. (1888.)

67. Contributors of papers are requested to send their papers to the Academy printer early enough to allow of their being put in type before the meeting, and read in proof. (1888.)

68. That on the evening of the day of meeting of the Sectional Council, when the papers for the next meeting have been decided upon, a circular be sent to each contributor informing him :—

(1.) That he is expected to be ready or else take his place at the bottom of the list.

(2.) That he must have an abstract ready with his paper, otherwise he will be noted in the published proceedings in such form as the Publication Committee think fit.

69. The General Council is empowered to defray the expenses in whole or in part of any illustrations which it may consider advantageous to the elucidation of the papers published by the Academy.

70. An abstract (prepared by the author) of each communication made at the Academy, along with a report of the discussions thereon, shall be furnished to the editors of such medical journals as may desire to publish them, and the authors of such communications shall be empowered to publish their papers *in extenso* in any periodical or periodicals they may think fit, such communications also to appear in the "Transactions," provided the Council consider them worthy of insertion.

Expulsion of Fellow or Member.

71. Expulsion of a Fellow or Member can take place only at a General Meeting of the Academy, on the motion of the Council, if two-thirds of the Members present shall vote for the same by ballot. Of such ballot the Council must give at least fourteen days' notice in writing to every Fellow of the Academy.

New Laws.

72. New Laws, or alterations in existing Laws, can be proposed only at the Annual General Meeting. Any Fellow proposing such alteration shall give notice to the General Secretary at least ten days before the General Meeting in October.

REPORT.

THE General Council have to report that the number of Fellows in the past session was 244; of Members, 28; and of Student Associates, 11. The Fellows have increased by 21, the Members have decreased by 6, and the Student Associates have increased by 9.

The financial statement shows that the Council have invested £100 in 2¾ per Cent. Consols, so that the total amount invested now stands at £596 2s. 4d. The balance in hands is £11 19s. 3d.

The Council deeply regret the great loss which not only the Academy but the Profession in Ireland have suffered by the sudden death of a former President of this body, Dr. Robert M'Donnell; and they beg to submit a copy of a resolution which was unanimously passed at their meeting on the 22nd May, 1889, and forwarded to Mrs. M'Donnell:—

> "That the President and Council of the Royal Academy of Medicine in Ireland have learned with deep regret of the death of Dr. Robert M'Donnell, F.R.S., the late distinguished President of the Academy, and they take this opportunity of expressing their sympathy with Mrs. M'Donnell and the family in their bereavement, and at the same time of placing upon record their sense of the loss which the Academy and the Profession at large have sustained by the removal of one whose many and varied attainments and world-wide reputation reflected lustre upon Medical Science in this country."

A resolution to a like effect was passed by the Surgical Section.

The Academy has also sustained severe loss by the death of Dr. Rutherfoord Kirkpatrick, Professor of Midwifery in the School of Physic, and a former President of the Obstetrical Section. A resolution of condolence was passed in the Section of Obstetrics, and was forwarded to Mrs. Kirkpatrick.

The General Meeting having a passed a resolution approving of the proposal that some member of the profession should be invited to address the Fellows and Members on some branch of medicine, the Council took steps to learn the wishes of the Fellows. In accordance with the replies received, they have invited Professor Purser to deliver a lecture during the present session, and they have agreed that 25 guineas shall be presented to him for his services.

The following new Rule has been adopted by the Council, and it is now submitted for sanction of the General Meeting:—

> "Whenever it has been agreed that a specimen exhibited at a Sectional Meeting of the Royal Academy of Medicine in Ireland shall be sent to the Reference Committee to report thereon as to its nature, the exhibitor is to retain the custody of the specimen until he shall be summoned to a meeting of said Committee to be convened by its Secretary, on an early day, when he will attend and submit it for examination."

The Council of the Section of Anatomy and Physiology sent forward the following communication:—

> "That the Council of the Royal Academy of Medicine in Ireland be requested to sanction the formation of a Committee in connection with the Anatomical Section, for the purpose of extending the method of collective investigation to anatomical research in the Schools of Ireland. Further, that they be asked to bear the necessary expense in connection with the carrying out of the scheme."

The General Council approve of this proposal, and they regard the scheme as one of the modes in which the Academy may aid in the development of Medical Science in this country.

S. GORDON, *President.*

W. THOMSON, *General Secretary.*

TREASURERS' REPORT.

RECEIPTS.

	£	s.	d.
To Balance in Bank, October 14th, 1888 -	17	6	10
„ Subscriptions - - - -	499	8	0
„ Dividends on Consols - - -	19	2	10
„ Overpayment by Fellow - - -	0	0	3
„ Fannin & Co., refunded on Illustrations -	0	19	10
Total Receipts - -	£536	17	9

EXPENDITURE.

				£	s.	d.
General Secretary - - - -				78	15	0
Printing, Stationery, Postage, &c. - -				62	15	11
Printing Vol. VI. and Postage - -				162	10	8
Illustrations in "Transactions" - -				13	15	11
Reporters - - - - - -				63	0	0
Attendants and Refreshments at Meetings -				35	17	11
Sundries - - - - - -				8	3	1
Invested in $2\frac{3}{4}$ per Cent. Consols - -				100	0	0
Balance in Bank - -	£10	8	3			
In Treasurer's hands - -	1	11	0			
				11	19	3
Total - - - -				£536	17	9

We have examined the Treasurer's Statement of the Accounts of the Royal Academy of Medicine for the Session 1888-89, and we find it correct.

F. W. KIDD, M.D.
JOSEPH M. REDMOND, F.K.Q.C.P. } *Auditors.*

October 9th, 1889.

WM. THOMSON, *General Secretary and Treasurer.*

Volume VII. of the "Transactions" has been forwarded to the following:—

Lancet	London.
British Medical Journal	Do.
Medical Press	Dublin.
Hospital Gazette	London.
Dublin Medical Journal	Dublin.
London Medical Recorder	London.
Edinburgh Medical Journal	Edinburgh.
Glasgow Medical Journal	Glasgow.
Liverpool Medical Journal	Liverpool.
Bristol Medical Journal	Bristol.
Asclepiad	London.
International Medical Journal	Do.
Annals of Surgery	Do.
Provincial Medical Journal	Leicester.
Birmingham Medical Review	Birmingham.
Sanitary Record	London.
Practitioner	Do.
College of Surgeons	Dublin.
Do.	London.
Do.	Glasgow.
Trinity College	Dublin.
Royal University	Do.
Queen's College	Belfast.
Do.	Galway.
Do.	Cork.
Royal Dublin Society	Dublin.
College of Physicians	Do.
Do.	Edinburgh.
Do.	London.
Faculty of Physicians and Surgeons	Glasgow.
Victoria University	Manchester.
University College	London.
Harveian Society	Do.
Pathological Society	Do.
Clinical Society	Do.
Medical and Chirurgical Society	Do.
Apothecaries' Hall	Do.
King's College	Do.
University	London.
Do.	Glasgow.
Do.	Edinburgh.
Do.	Durham.

University	St. Andrews.
Do.	Aberdeen.
Do. College	Dundee.
Do.	Melbourne.
Do.	Sydney.
Do.	Adelaide.
Do.	Calcutta.
Do.	Bombay.
Do.	St. Petersburg.
Do.	Paris.
Do.	Vienna.
Do.	Berlin.
Do.	Brussels.
Do.	Bologna.
Do.	Madrid.
Do.	Amsterdam.
Do.	Christiania.
Do.	Stockholm.
Do.	Toronto.
Do.	Quebec.
Do.	New York.
Do.	Philadelphia.
Do.	New Zealand.
Do. Library	Tokio, Japan.
Medical Institute	Birmingham.
Dr. Ashby	Do.
Director-General Billings	Washington, U.S.
Journal de Médecine et de Chirurgie (M. Lucas Championière)	Paris.
Archives de Chirurgie	108 Boulevard St. Germain Paris.
Archiv für Klinische Chirurgie	Berlin.
Centralblatt für die medicinischen Wissenschaften	Do.
Zeitschrift für Chirurgie	Do.
The Australian Medical Gazette	Sydney, N.S.W.
Spitalul	Bucharest.
New York Medical Journal	New York.
Journal, American Medical Association	Chicago.
Bulletin de l'Académie Royale de Médecine de Belgique	Brussels.
Medical Journal	Brooklyn.

BOSTON MEDICAL LIBRARY ASS
MAY 2 1891

TRANSACTIONS
OF THE
ROYAL ACADEMY OF MEDICINE IN IRELAND.

SECTION OF MEDICINE.

ON MORPHINISM.

By ARTHUR WYNNE FOOT, M.D, Univ. Dubl.; F.K.Q.C.P.;
Senior Physician to the Meath Hospital;
Professor of Medicine, Royal College of Surgeons, Ireland.

[Read in the Section of Medicine, November 15, 1889.]

The opium habit is at present, in civilised countries, indulged in almost exclusively through the instrumentality of the hypodermic syringe.

Opium *smoking*, which has long been a national characteristic of the Chinese, has found a limited number of imitators among the lower strata of society in places where Chinamen have introduced the practice.

Opium *eating* is said by competent observers to be very much on the increase in the manufacturing districts of England, and in the counties of Lincoln, Cambridge, and Norfolk.

Early in the present century, though there were occasionally cases of people who used opium to excess in the solid form or in solution, the use of morphin by the subcutaneous method was absolutely unknown. It was reserved for quite modern times—since about the year 1859—to see the subcutaneous method employed as the principal, and almost the sole, channel for the vicious introduction of morphin into the system, while the terms

morphinomania (first employed by Charcot) and morphinism (first employed by Levinstein) are so recent as hardly yet to have come into general use. The misuse of morphin has certainly been accentuated by a knowledge, on the part of the public, of the hypodermic method of administration, and of the rapid results which follow its employment.

The use of remedies by hypodermic or subcutaneous injection is an outcome of the British school of therapeutics. Two men of inventive mind—Mr. Charles Hunter of London, and Dr. Alexander Wood of Edinburgh—have each claimed the origination of this method. I have often heard the late Mr. Francis Rynd state in the Meath Hospital that he was the first to employ it. The truth of the matter appears to be that the first attempts at hypodermic medication were made by Mr. Rynd[a] in 1845, while to Dr. Alexander Wood, in 1855, is due the popularisation of the method. It was Mr. Charles Hunter who gave the name of hypodermic to the subcutaneous method.

The opium habit, in the form of the hypodermic use of morphin, is more general and more on the increase than many think, and is indulged in rather by the refined and educated classes than by the lower orders. Many notices of its prevalence have lately appeared in America and on the Continent. According to Jouet the habitual injection of morphin is to day, in France at least, almost a matter of fashion, and presents of pretty syringes in silver cases are exchanged on festive occasions. Zambaco, whose observations were made in Constantinople, says that the ladies of the better classes wear jewelled cases containing hypodermic syringes and artistic bottles for the morphin solution, and avail themselves of convenient opportunities to make reciprocal injections of morphin.

Morphinism has a particular interest for medical men, because quite an incredible number of our colleagues have fallen victims to it. Of Levinstein's 110 cases, 47 occurred in persons belonging to the medical profession, or dependent on it: thus, 32 physicians, 8 wives of physicians, 1 son of a physician, 4 nurses, 1 midwife, and 1 student of medicine, were among his patients. The acquisi-

[a] Dubl. Med. Press, 12th March, 1845. P. 167.

tion of the morphin habit has often been laid to the charge of the medical attendant, who has been blamed for imprudent use of the syringe, or indiscriminate employment of morphin. These charges have been made by some who are entitled to speak with authority. For instance, Niemeyer, alluding to the morphin habit, observes this practice is not uncommon, and, as it is generally begun under the physician's recommendation, we may consider ourselves as responsible for its cure. Obersteiner, speaking of the prevalence of morphinism among the profession in Austria, hints broadly that, in his view, there is poetical justice in the fact that this fatal habit should work most havoc among the class of men who, by carelessly prescribing the drug, and instructing in the use of the hypodermic syringe, have been mainly instrumental in disseminating it.

The *origin of the habit* may, perhaps, as fairly, and, at all events, more charitably, be traced to some one or other of the following causes—(1) Relief of pain; (2) insomnia; (3) melancholia. Besides these conditions, each of which is frequently directly relieved by the use of morphin, simple *curiosity* has laid the foundation of the habit. Obersteiner gives the case of a physician who was incredulous as to the rapturous ease which a cancerous patient alleged he had from morphin, and took one dose himself for experiment. This dose awakened a ravenous appetite, which he could never afterwards overcome. One of Dr. B. W. Richardson's patients told him he began the practice simply to ascertain what the effect was like, and that, finding it not unpleasant, he went on until he felt he could not live without its continuance. Mere *imitation* has been alleged as the cause of the habit by one who could give no better reason for it than that a friend who lived in the house with him had recourse to it, and that he "followed suit."

A receptivity for morphin is, fortunately, not the property of every constitution. It is only on certain individuals that it operates so as to produce intellectual excitement or psychical pleasure, and by creating an inappeasable hunger for a repetition of the dose, renders these individuals morphin habitués. With many even small doses cause such unpleasant effects that they have no wish to renew the experiment. Cases are well known of chronic illnesses

which have required large doses of opium for a long time, often for years, yet the patients do not fall victims to the opium habit. It is, however, a drug not to be trifled with rashly; for the morphin habit has established itself in as short a time as a month in persons who are able to take it without feeling any of its ordinary disagreeable consequences.

As a general rule, but one to which there are exceptions, the shorter the time it has been indulged in the easier it is to cure. The morphin disease is also more amenable to treatment if it has not been preceded by alcoholic excess; for, if so, the alcoholic habit intensifies the narcotic craving, and adds much to the difficulty of treating it. Dr. B. W. Richardson says he has never met with the morphin habit among total abstainers from alcohol. However, on the other hand, it is stated that the abuse of morphin has, in many cases, replaced the abuse of alcohol, especially in refined society, and that such is particularly the case in some districts where rigid total abstinence principles have been largely adopted by the inhabitants.

The quantity of the drug injected by a morphinist varies greatly. Few are satisfied with anything less than 3 grains in the twenty-four hours; many vastly exceed this amount: 15 or 20 grains are not uncommon quantities. In a case reported by Dr. Wm. Stuart of Hawkes Bay, New Zealand, the regular quantity used daily for some months was one ounce of the hypodermic injection of morphin (B.P.), which means a daily dose of 40 grains of acetate of morphin. The estimate of the amount was based on the statements of the patient and his wife, checked by the known quantities supplied by different druggists. It may here be observed that it is often difficult to ascertain the exact amount of morphin which is used, because morphinism like alcoholism induces a total disregard for truth, and, in most cases, habitual morphinists become such liars that a knowledge of this particular phase of psychical degradation is necessary to rightly estimate their statements. One of Obersteiner's female patients told him herself that no one should believe a morphinist, as they were all alike untruthful.

The inquiry is a very natural one—what becomes of those

immense quantities of morphin, and how is it that acute opium poisoning is so rarely observed? In reply, it may be stated that the toleration of individuals is so variable, that what is a large dose for one is a moderate dose for another; moreover, the maximal doses have been arrived at by degrees, and are not initial ones. The following possibilities may also be suggested in explanation—that a process of accommodation may be set up in the tissues, resulting in a lessened degree of susceptibility, while, at the same time, with a slower absorption a more rapid excretion may be established. Furthermore, morphin is known to be eliminated with considerable rapidity by the kidneys, and a certain amount is also excreted by the mucous membrane of the stomach. Diedreich assumes that a portion of the morphin introduced into the organism is converted into oxydimorphin, or other analogous substances, which have the property of counteracting to some extent the toxic effects of the drug. Whatever becomes of it, the fact is patent that the daily use, subcutaneously, of lethal doses of morphin may be persevered in for a length of time without the appearance of the symptoms of acute opium poisoning.

Diagnosis.—The question of diagnosis can hardly be said to arise when the habit is admitted and the applicant professes a willingness to give it up and an anxiety to be cured; but, even in such cases, there is a strange inconsistency in behaviour, as, in a case which occurred to myself of a young medical man, who, while lamenting his infatuation, and professing his desire to be cured, took a charged syringe from his pocket and injected himself through his trousers without waiting to strip his leg. This conduct was very similar to that of a lady under Dr. Richardson's care, who was dotted all over those parts of her body that were within her reach with the marks of the punctures she had made, who used to beg, while imploring him to get her out of the practice, to allow her to show him how she did it. Not unfrequently the real cause of the ill-health produced by morphinism is unexpected by the family or friends of the patient, or the patient intentionally deceives the doctor by a total denial of the practice, or, if forced to admit it, misstates the amount or frequency of the dose. In such

cases it is rather a question of *detection* than of *diagnosis* and corroborative evidence of much importance is afforded by information that the patient procures morphin himself or through others, by the presence of the puncture marks of the hypodermic needle, or by the discovery of morphin in the urine. This latter point is not such an easy matter to determine, and as it requires an elaborate chemical examination the urine should be submitted to none but a competent analyst. A diagnosis in morphinism has been derived from a study of pulse tracings. At a meeting of the Académie des Sciences, MM. B. Ball and O. Jennings made a communication to show that the pulse is normal during the period of satisfaction. When the patient begins to feel renewed craving the pulse presents a flat elevated surface, which indicates the diminution of cardiac pressure, and explains the sensation of weakness experienced by the patient. The presence of this flat surface may be useful in the diagnosis of morphinism (*Brit. Med. Jour.*, May 7th, 1887, p. 1003).

Even when the morphin habit is suspected or admitted, the amount or frequency of the dose can rarely be fully known, because one of the mental peculiarities of morphinists is secretiveness concerning the vice, and they invariably deceive those about them in regard to the details of their indulgence. Individuals above reproach in other matters, and previously of unquestionable veracity, lie without any hesitation in this matter. A patient under Dr. Wilson's care, who had secreted in her room a quantity of morphin, when about to undergo treatment denied either having taken or then having in her possession any opium or morphin whatever, using the expression, "I call God to witness that I neither now have, nor have had since I began the treatment, any preparation of opium or morphia whatever." Within ten minutes afterwards sixty quarter-grain pills of morphin were discovered secreted under her bolster. This patient was a devout, refined, and, in regard to other matters, a trustworthy person.

Prognosis.—The prognosis in morphinism is not so favourable as was at one time believed. Von Boeck is of opinion that the morphin habit belongs to the category of diseases which are

almost incurable. The weaning from it is a laborious task for the patient as well as the physician, and yet thereon rests the only hope of recovery. It is considered by those who have had the largest experience of such cases to be easier to cure a morphin eater of his craving than a morphin injector. The probability of a cure may be estimated by attending to the following points:—(1) The duration of the habit—cases of short existence being more successfully treated than those in which the habit, of long standing, has exercised a deleterious influence on mind and body; (2) the persistence or not of the condition which gave rise to the use of morphin, for if this condition is irremovable a cure is scarcely to be expected; and (3) the physical and nervous constitution of the victim; because very weakly individuals fall into such a state of prostration on the withdrawal of their accustomed dose that it is necessary to give it again; and because intense psychical disturbance follows its stoppage in others of specially nervous temperament. Morphinism has been classed by some—as Laehr and Fidler—among the psychoses, but it is rather the expression of a morbid constitution than a substantive affection in itself; hence it is that nervous subjects, invalids, and individuals deficient in moral and physical tone, are specially prone to it.

The prognosis is not much affected by the magnitude of the daily dose, except in so far as the larger doses indicate chronicity in the habit which in itself is an unfavourable point, even in the case of small doses. In Obersteiner's experience the prospect of a permanent cure is distinctly gloomy, unless all the conditions just now mentioned are fulfilled.

It is a hopeful consideration that in the majority of cases of morphinism to break the habit means to get well, because, unlike alcoholism, the morphin habit does not entail structural lesions of any vital organ. In relation to prognosis it is well to bear in mind how frequent, in the experience of all, are relapses. They are more common in men than in women, and in persons of feeble physical and mental organisation than in those possessed of bodily and mental vigour. Of 82 men treated by Levinstein relapses occurred in 61; of 28 women, in 10; of 38 physicians, in 26. The danger

of relapse diminishes with the lapse of time; yet a single dose of morphin or a hypodermic injection may, after an abstinence of months, precipitate a relapse. A medical man, after four attempts at weaning, was treated in Obersteiner's institution, and dismissed apparently well, yet, after three months, he was again in his former state. So great is the tendency to relapse that Jaeckel does not consider a cure to be accomplished by the mere suppression of the morphin hunger. He lays great stress on a continuance of control over the patient in a proper institution, in order to guard as much as possible against relapses. Erlenmeyer contends that the longer a patient, after the discontinuance of the habit, continues under control in an institution the more favourable will be the prognosis of a permanent cure. He does not deny the possibility of successful treatment under propitious circumstances outside an institution; but he is quite opposed to the view of Wallé that treatment in institutions is not only unnecessary but even injurious.

Treatment.—Emancipation from morphin-slavery is unattainable by voluntary effort. Jaeckel is unaware of any successful case of self-treatment in morphinism, and does not advocate its being attempted, even in the case of medical men. The difficulty of breaking the morphin habit is greater in some cases than in others. It is considered to be easier to cure a morphin eater of his infatuation than a morphin injector. The process of cure, or of what Charcot calls demorphinisation, requires a degree of moral and physical courage seldom at the disposal of a morphin habitué. In the case of the highly gifted and more delicately organised, a fiery trial must precede deliverance. The abject mental state of the morphinist calls for firmness, gentleness, and tact on the part of the physician and the attendants. Neither the intensity of his craving, nor the reality of his sufferings should be underrated or disputed.

The methods of treatment may conveniently be considered under four heads—(1) the *deception* plan; (2) the *substitution* plan; (3) the *tapering off* plan; (4) the *abrupt withdrawal* plan. The first two methods—the deception and the substitution plans—may be dismissed as hardly worth serious consideration in the management of

confirmed morphinism. The deception plan, which consists in injecting water, or some solution not containing morphin, seldom does good for any length of time, and is often not even of momentary use to a real morphinist. The injection of a few drops of water, or the very prick of the needle, or even of a pin instead of it, may relieve slight local pains in very nervous or imaginative people, especially if a successful result has been emphatically predicted. In a morphin injector the association of ideas may, perhaps, lend a temporary charm to the sight of the hypodermic instrument, for I have seen the eyes glisten and depression vanish from the features upon the production of a syringe which contained no morphin; but the morphinist is not deceived a second time, and the indignation of one who feels he has been cruelly hoaxed is sure to replace any confidence that might have been reposed in the attendant who adopts the deception plan. The victim of morphin hunger is fully sensible that when he begged for bread he has been given a stone.

The substitution plan, which consists in replacing the morphin by some other drug, such as cocain or chloral, has been aptly described as handing the sufferer over from one enemy to another. The substituted drug sometimes produces even greater havoc than the one it has replaced, because of its introduction into a system already undermined by another toxic agent. Cocain is the drug which has been most championed as an antidote for the morphin habit. On the subject of the treatment of morphinism by cocain, Erlenmeyer has come to the following conclusions:—(1) That its use does stop the morphin craving; (2) that this effect is very transitory, lasting only from 10 to 25 minutes; (3) that cocain is a real substitute for morphin and not an antidote; (4) that it should not be given, as its effect is so temporary and its use dangerous. Moreover, there is great risk of developing a habit—cocainismus—which is worse than morphinism. (Brain, x., 533).

The treatment then is narrowed to a choice between the tapering-off plan and the abrupt withdrawal plan, and one or other of these methods is advocated by those who have found the particular course they adopted of use in special cases. There is no unanimity of

opinion as to which mode of treatment should be employed, and, before coming to a decision on this point, the following matters have to be taken into consideration—viz., the duration of the habit; the dose which has become habitual; the cause of the habit; the effects it has had on the patient; and his strength of constitution. It by no means follows that the adoption of either method will be ultimately successful in any given case, so far as the cure of the practice is concerned, for it must be admitted that with the best efforts there are many failures to break the threefold cords of the morphin habit; but, nevertheless, failing cure, the patient may be restrained from resorting to increased doses, or the amount may be got reduced from a dangerous to a comparatively safe quantity. Whichever plan is adopted, the advantages are not to be overlooked or forgotten of careful regulation of the diet, change of air and scene, cheerful society, and the administration of tonic remedies.

The tapering-off plan consists in the gradual reduction of the dose, until none of the drug is required. For instance, a solution of morphin is made of known strength—say, so that each 20 minims is equal to one grain of morphin; then, after each 20 minims are taken from this solution, 20 minims of distilled water are added to it, so that the solution becomes gradually weaker. An analogous tapering-off or weaning plan is adopted in the East in the case of opium chewers, by gradually mixing an increasing proportion of wax with the crude opium, till at last there is more wax than opium in the combined mass. Similarly in treating opium smokers, they mix more and more tobacco with the opium before ignition. In the tapering-off plan the patient should not be informed of the progressive diminution of the dose, for some, if acquainted with the details of the process, feel quite distinctly the withdrawal of a small fraction of a grain. Dr. B. W. Richardson considers it better to reduce the dose at each administration than merely to lessen the number of injections in the 24 hours. No tricks are to be played with the patient by injecting water or solutions devoid of morphin, for such behaviour destroys confidence. Erlenmeyer considers the tapering-off plan of treatment unsatisfactory because of the continuous supervision it requires on the part of someone who can be

implicitly relied on to provide against the surreptitious use of morphin; and he thinks it further undesirable on account of the prolonged misery it entails, because the duration of the weaning process must be proportionate to the dose which has become habitual. (Brain, x., 533.)

The abrupt discontinuance of the drug (the method of Levinstein as it is called from one of its principal advocates) is attended in all cases by indescribable sufferings, and in many by serious dangers, among which collapse and a condition resembling delirium tremens are to be specially considered. Von Boeck, who approves of this method, considers the most effectual plan is to correct the habit suddenly, *uno ictu*, if we have to do with hardy individuals, admitting meanwhile that the great danger in this method is collapse, which he advises should be obviated by the copious use of wine.

When morphin is suddenly cut off in those accustomed to its use, a remarkable group of symptoms ensue, called the *Abstinenz Symptome*, or reactionary effects. They are severe and alarming. As enumerated by Obersteiner, they comprise acute diarrhœa; insomnia; great excitement, amounting at times to mania dangerous to those around, and particularly to the physician; hallucinations; and collapse. The *Abstinenz Symptome* are liable to occur under the tapering-off as well as under the abrupt withdrawal plan, but are seldom so severe under the former. This so-called reactionary stage may last from four to eight days, and, according to Von Boeck's description, the patient looks ill and emaciated, the eyes heavy, the respiration laboured; they suffer from dyspnœa, anxiety, palpitation, frequency of pulse, up to 120, perspirations, great irritability, dysuria, pains in all possible nerve regions (especially in the stomach and intestines), anorexia, thirst, diarrhœa, complete insomnia, and this state of things may terminate in death. The abrupt withdrawal of alcohol or chloral is possible and safe, but in the case of morphin it occasions the direst symptoms both objective and subjective, which nothing will relieve but morphin. Erlenmeyer, struck with the gravity of these *Abstinenz Symptome*, and particularly with the tendency to collapse, discards the system of sudden withdrawal adopted by Levinstein, and recommends a

combination of the third and fourth methods—namely, the complete withdrawal of the whole quantity of morphin, not abruptly, but in from six to twelve days. By this compromise he hopes for a successful result, absolute freedom from danger, and a shortened period of suffering. It seems certain that, with enfeebled individuals, the amount of the accustomed dose must be gradually reduced, and in such cases the tapering-off plan is preferable to that of abrupt withdrawal. The abrupt withdrawal plan is most applicable when the morphin habit has not been confirmed by long use, and the dose injected has not been excessive. Erlenmeyer records some curious facts, showing that children born of mothers addicted to morphinism are affected just in the same way as if the drug had been directly administered to them and suddenly withdrawn. These infants are, in fact, congenital morphin habitués, and become liable to serious troubles, such as complete insomnia for as long as sixty hours (Jouet, p. 41) as soon as they are separated by birth from the source of their constant supply.

The *Abstinenz Symptome*, or reactionary effects, are liable to occur upon the sudden stoppage of the accustomed dose, whether the habit has been of long or short duration. They are more violent, and developed with greater rapidity, in some cases than in others. Some patients rest quietly in bed, enduring with fortitude sufferings from which there is no escape; others, silent and apathetic, present the appearance of abject despair. With the greater number restlessness is continuous, and very often extreme; they are with difficulty kept in bed—if left to themselves they run frantically about the room, moaning, bewailing their condition, and imploring the attendants for that which alone is capable of relieving their distress. This stage of excitement may gradually subside, giving way to a state of the most profound exhaustion, or it may develop into a condition which, being accompanied with hallucinations, delirium, and tremor, closely resembles the results of alcoholism. At times it is necessary to have recourse to physical restraint, as in a case reported by Von Boeck of a young doctor who could be cured of giving himself morphin injections only by being actually shut up in a room for more than a week. He resisted like a

maniac, scratched at the walls with his nails, wept and shrieked from misery, ate nothing, was unable to sleep, and had diarrhœa. At last, after some days of untold suffering, he began to feel better—to eat and sleep. From that time his weight rapidly increased, his appearance grew fresh and healthy, and he recovered his taste for tobacco, so that he smoked and drank beer as in the days of his health. Having regard to the likely occurrence of these *Abstinenz Symptome*, the desirability of conducting the treatment away from the patient's home can scarcely be questioned, and it is a fact that cases in which the home treatment of morphinism have proved successful are certainly exceptional.

The sudden interruption of the morphin habit by acute disease may give rise to very puzzling symptoms. Attention has been drawn to this matter (see *Lancet*, 1887, Vol. II., p. 300) by M. Richardière, in relation to the case of a physician, a morphin injector, who got pneumonia. Most alarming symptoms of dyspnœa, cyanosis, weak and irregular heart-action and excessive rapidity of pulse, caused the greatest anxiety for the result. M. Richardière believes that a fatal issue was prevented only by a recognition that these were the *Abstinenz Symptome* of a morphinist, rather than due to the pneumonia, and by a prompt recourse to the stimulant for which the system was craving.

In cases where the habit is denied or concealed Charcot relies for the diagnosis on the occurrence of the characteristic *Abstinenz Symptome* which supervene when the drug is withheld during the process of demorphinisation.

The assistance to be obtained from drugs in conquering the opium habit is maintained by Dr. Fleming, who advocates the sudden suspension of the habit as infinitely more efficient and easier to the patient than the gradual diminution of the dose. He recommends the administration of large doses of phosphoric acid and lupulin to counteract the immediate effects of the suspension of the drug. Having interdicted all use of opium and taken the necessary precautions against its being obtained surreptitiously, he orders—Acidi phosphorici diluti, ʒx; tincturæ lupulinæ, ʒxxx.—M. Of this two drachms are to be taken in a large wineglassful of water every four

hours. The dose may be increased or lessened according to circumstances. If insomnia is very protracted, he gives, at bed-time, the following draught:—℞. Tincturæ cannabis Indicæ, ♏. xxx ad lx; spiritûs ætheris, ʒi; aquæ sambuci, ad ℥ss.

In the case of a gentleman addicted to morphin for seventeen years, and who had sometimes taken thirty-two grains in the day, Dr. Seymour Sharkey found the most effectual means of quieting the extreme restlessness, which occurred upon the sudden withdrawal of the morphin, to be grain doses of extract of belladonna as suppositories, with ʒi doses of tincture of hyoscyamus. A detailed account of this very remarkable case, in which a cure of the habit seems to have safely been accomplished by the sudden stoppage of the drug, when used in enormous excess, will be found in the *Lancet.*[a]

The weaning process is facilitated in some by the administration of ammoniated alkalies, bitters, or ethereal substances. Thus, Coleridge experienced sensible relief from the use of ammoniated tincture of valerian.

Dr. Oscar Jennings, of Paris, from his sphygmographic observations on the pulse in morphinism, deduces the necessity of using, in the treatment of such cases, a circulatory stimulant of some kind to replace the withdrawn morphin. Of such medicines he has found nitro-glycerine to produce the same improvement of the pulse as morphin, and patients taking it declare the sensation of warmth and comfort following its use is like that which they experience on first receiving morphin after deprivation of it.

Great care is needed in the case of pregnant women addicted to opium, because incautious attempts to withdraw the habitual drug are, according to some authorities, almost certain to be followed by the loss of the fœtus. That this is not always so is shown by a study of Braithwaite's case of the abrupt plan of treatment in a young woman in the habit of using large doses of morphin. During the demorphinisation of a pregnant woman, a patient of M. Tarnier, the movements of the fœtus became so lively, and the uterine colic so sharp, that the process had to be interrupted (Jouet, p. 41).

[a] Vol. II. 1888. P. 1120.

Strange as it may at first sight appear, in the treatment of morphinism the question may arise, is it right or necessary to interfere at all? The decision in such a case turns upon the motive which led to the commencement of the practice. In the large majority of cases the alleviation of physical pain is alleged as the excuse; but if the person be a sufferer from malignant disease beyond the range of surgical skill, and if his pain be relieved, and life rendered endurable by habitual injection of morphin, there is little more interference called for than such as would lead to a systematic regulation of the dose within the limits of safety. The same course may be adopted in a case where symptoms of diabetes or heart disease are favourably controlled by the use of what would otherwise be excessive doses of morphin. In such cases it is impracticable, nor would it be desirable, to interfere with the habitual use of morphin. It may be even advantageous in some cases to acquire the habit. Dr. Lutaud believes that the methodical subcutaneous injection of strong doses of morphin, from two to nine grains daily, is highly beneficial in cancer of the uterus. It checks the hæmorrhage, causes the pain to disappear, acts as a necessary stimulant to the patient, and prolongs life. His observations in this direction were suggested by his remarking that in female morphinists menstruation becomes suppressed directly the dose of morphin reaches a certain strength. (*Brit. Med. Jour.*, 5th Nov., 1887. P. 1,018).

It is noteworthy that a liking for morphin, amounting to what has been actually termed morphinomania, has been noticed several times in some of the lower animals—in cats and especially monkeys. A physician in Cambodia, M. Ludovic Jammes, communicated to the Académie des Sciences, Paris, through M. Vulpian, the case of some monkeys belonging to an opium smoker. These animals lived habitually with him in an atmosphere saturated with the products of the combustion of opium, and, moreover, were fond of eating the residue left in their master's pipe. It was observed that during prolonged absence of this man the animals, after four or five days, became languid, and showed all the symptoms of depression usual in demorphinisation. On the return of their master the

monkeys gladly resumed their habits, and recovered their good humour. The facts stated did not astonish M. Vulpian, who presented the communication to the Academy, under the title of "Morphinomania in Animals." (*Lond. Med. Rec.*, 15th August, 1887. P. 374.)

Levinstein,[a] in a recent paper on the "Convalescence" from morphinism, states that, were more attention paid to this critical period, fewer relapses would take place. The whole therapy of the convalescence after the withdrawal of morphin or cocain may be summed up in careful oversight of the patient, strict abstinence from alcohol, and the avoidance of all unpleasant psychic impressions. It is a notable fact that when several persons under treatment for morphinism are together in an institution for the care of such patients, they are very apt to associate with one another, to talk freely of their affection, and often to combine in deceiving the physician in charge. This association should be prevented, for there is nothing more ruinous to the recovery of the lost will-power than to allow the thoughts or conversation to run on morphin.

There are other directions in which the subject of morphinism presents points of interest—for example, in relation to medical jurisprudence and to life assurance; but the limits of the present communication forbid me from entering upon their consideration.

Appended hereto is a list of some of the more important publications on the subject of morphinism:—

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Braithwaite, James, M.D.—"Lancet." 21st Dec., 1878. P. 874.

Burkhardt.—"Die chronische Morphium-Vergiftung." Bonn, 1877.

Busey.—"Philadelphia Medical Times." 1876.

[a] Deutsch. med. Wochenschr. 1888. 35, 715. Inter. Journ. Med. Sci. 1888. P. 622.

Calvet.—Essai sur le Morphinisme. "Thèse de Paris." 1876.

Erlenmeyer.—Die Recidive der Morphium-Sucht. "Centralbt. f. Nervenheilkunde." 1879.

Fiedler.—Ueber den Missbrauch der Morphin-Injectionem. "Zeitsch. für prakt. Medicin." 1874.

Fleming.—"British Medical Journal." 15th Feb., 1868. P. 137.

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Kormann.—"Deutsche med. Wochenschrift." 1877.

Krage.—"Ueber Albuminuria und Glycosuria nach Morphium." Griefswald, 1878.

Kunz.—Die Morphiumsucht. Baier, "Ærtz. Intelligenzblatt," 1876.

Laehr.—Ueber Missbrauch mit Morphin-Injectionem. "All. Zeitsch. für Psychiatrie." 1872.

Leidesdorf.—Die Morphiumsuch. "Wien. med. Wochenschrift." 1876.

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Moinet.—On the Administration of Opium. "Edin. Med. Journ." 1875.

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Richardson, B. W.—Morphia Habitués and Their Treatment. "Æsclepiad." 1884.

Richter.—Casuistik zum Morphinismus. "Berl. klin. Wochenschrift." 1876.

Sharkey, Seymour, M.D.—"Lancet." 1888. Vol. II. P. 1,120.

Stuart.—"Brit. Med. Jour." 11th May, 1889. P. 1,051.

Whipham, Thomas M.B.—"Clin. Soc. Trans." London, 1875. Vol. VIII., p. 108.

Wilson, James C., M.D.—Article on the Opium Habit and Kindred Affections. Vol. V., "Pepper's System of Practical Medicine."

Zambaco.—"De la Morphéomanie." Paris, 1883.

CASE OF EXTRA-PLEURAL ABSCESS SIMULATING EMPYEMA.

By WALTER G. SMITH, M.D.
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[Read in the Section of Medicine, December 13, 1889.]

THE case which I wish to bring forward is, I think, deserving of record, on the ground of its unfrequent nature, and of the difficulties which attended its diagnosis. Indeed, various opinions as to its pathology were expressed by those who saw the case before and after the patient came to Dublin, and certainty as to its true character was attained only by the light of the *post-mortem* examination.

On October 20th, 1888, I was asked to see Mr. —., a gentleman from the South of Ireland. He was thirty years of age, and his history was that of a strong and healthy man, active in his profession, and fond of out-door exercise, especially yachting. Eighteen months previously he had had a slight attack of pleurisy on his right side. He considered himself to be in excellent health until six weeks before I saw him, when he was seized, while on board his yacht, with a sudden sharp pain in the lower part of the right side. There was an indistinct account of his having received a blow or hurt from a spar on that side recently. He sought medical advice, and was told in one quarter that he was suffering from pleurisy, and in another from hepatic affection.

He was put under active treatment; nevertheless, the pain continued; he felt poorly, and lost strength. He then arranged to come up to Dublin, and, upon examination, he exhibited the signs, apparently, of a moderate pleural effusion on the right side. There was complete dulness up to the angle of the scapula, the respiration was feeble or extinguished, and vocal fremitus was lost. From the first there was marked *tenderness* along the eight and ninth ribs.

He looked haggard and worn, and had a moderate degree of pyrexia. Throughout his entire illness the temperature was of a remittent type, usually fluctuating between 100° and 101°. Another persistent symptom was frequency of pulse; it rarely fell below 120. His appetite was tolerably good, alothugh the tongue was red and glazed, and he was tormented with flatulence. After a while these symptoms mended, but recurred later on, and caused a good deal of annoyance.

For diagnostic purposes an exploratory puncture was made over the right side, posteriorly, with negative results. Dr. James Little saw the patient with me, and thought it probable, from the localised tenderness over the ribs, there was disease of the bone. Next day I again punctured, and gave exit to a few drops of fœtid, bloody pus.

After this Dr. Purser's opinion was obtained, and we agreed that it was advisable to put in a drainage-tube. Accordingly, Dr. Charles Ball was called in; and when the abscess-cavity was freely opened, about a pint of fœtid, sanious pus escaped, and a portion of rib was found to be necrosed. A drainage-tube was inserted, and the case was dressed with great care from day to day. Notwithstanding easy drainage and copious irrigation with antiseptics (permanganate of potassium), the foul, bloody discharge persistently flowed on. Gradually a prominent swelling developed in front of the lower edge of the liver. This soon gave way, the purulent discharge being profuse, but variable in amount. A probe passed in for several inches downwards, and also directly backwards.

About this time Dr. Bennett saw Mr. —. with us on several occasions. The hectic fever became more marked; there were occasional sweats, and the patient's strength commenced to run down; the bowels were very costive; the urine was dark—it never contained bile or albumen.

Early in December the wound in which the drainage-tube lay assumed a very unhealthy appearance. Its edges became excessively painful and tender; ulceration advanced steadily, disclosing a ragged looking, sloughy cavity. Other spots in the adjacent skin sloughed out; these speedily coalesced, so that, ere long, a huge

sloughing ulcer presented itself, of the form of a figure of 8. Along the lower ribs a dusky-red and very firm swelling uprose, looking like a malignant tumour. The final scene soon came, and the patient died January 18, 1889.

Post-mortem examination, Jan. 20th.—The side of the chest exhibited an enormous black gangrenous cavity, from which projected the remains of two or three necrosed ribs.

Thorax.—Right lung partially adherent. A few specks of recent pleurisy on the lower part. *Neither pleural cavity contained fluid.* The right lung was emphysematous in its upper part, the left lung along its anterior and inferior edges; they were otherwise healthy. The heart and pericardium were quite healthy. The ribs, from the seventh rib downwards, were thoroughly rotten.

Abdomen.—Here was found an immense sloughy abscess which had destroyed the right half of the diaphragm, but left the pleura intact. The liver and gall-bladder were normal. The cavity of the abscess was bounded by the pleura above, and by the abdominal integuments in front, and it was observed that the skin was neither œdematous nor discoloured. The posterior wall of the abscess was formed by the liver, colon, and the right rectus abdominis muscle, which was black and soft, so that the purulent discharge had dissected its way between the skin and muscles, forming a long narrow pouch downwards. The colon was adherent to the liver. The spleen was soft, of moderate size. There was no diffuse peritonitis.

Looking back upon the case, and with the guidance of the *post-mortem* examination, it seems reasonable to conclude that the starting-point of the whole illness is to be referred to the ribs, and that the bone mischief was, probably, due to direct injury.

Suppuration slowly ensued—first, in the thoracic walls; then, creeping downwards, it corroded the diaphragm, and penetrated into the abdominal wall, dissecting the rectus abdominis from the flat muscles in front.

Although the diagnosis of empyema proved to be incorrect, the error may be admitted to have been an excusable one; and it is, at all events, instructive to recall how closely the physical signs of the parietal abscess mimicked those of a pleural effusion.

This case adds another to the list of obscure cases upon record in which errors in diagnosis have been made as to the nature and exact seat of collections of fluid in the lower part of the thorax or in the upper regions of the abdomen.

It is, for instance, by no means always easy to distinguish between supra- and infra-phrenic effusions, and, on the right side, abscess between the liver and diaphragm has frequently been mistaken for hepatic abscess—similarly with perinephric abscess. (*Cf.* an interesting paper by Dr. Hilton Fagge, Guy's Hospital Reports, 1874).

Another point deserving of notice is the remarkable resistance which the two great serous membranes, the pleura and the peritoneum, oppose to the invasion of suppuration from without. Notwithstanding their proximity to a collection of fœtid and gangrenous material, and this for a long time, neither membrane was affected to any extent with recent inflammation, and neither serous sac contained fluid.

That extra-pleural abscess may penetrate the pleura appears from an instructive case recorded by Dr. Halley in the "Transactions of the Pathological Society of London, 1855."

NOTE ON THE PRACTICE OF CLINICAL MEDICINE IN EUROPE IN THE YEAR 1507.

By JOHN KNOTT, M.A., M.D. (Dubl.); M.R.I.A.

[Read in the Section of Medicine, January 24, 1890.]

THE subject of this note was suggested to me by the perusal of the accompanying volume [exhibited]. It is the property of my venerable and learned friend Professor Aquilla Smith, who believes it to be the earliest printed record of the practice of clinical medicine in Europe. It bears the title, "Antonii Benevenii Florentini Medici et Philosophi de abditis nonnullis ac mirandis morborum et sanationum causis," and saw the light at Florence in the year A.D. 1507. The fact of the production of such a volume at such a date is highly interesting, not only in the history of the advancement of learning and science, but in the general relationships of that period to the social and artistic progress of man. It was precisely at the time of the printing of this volume that Florence had attained the zenith of her beauty and her glory, from which she was to decline so soon and so sadly. It is a product of the age and of the surroundings which produced the creations of Raphael and of Michael Angelo, which had been actively impressed by the frenzied enthusiasm of Savonarola, were greedily imbibing the political principles of Machiavelli, and had, at that precise date, just ceased to be the arena of the diabolical machinations of Cæsar Borgia. A native of this same city of Florence had recently given his name to the newly discovered Western world, and the celebration of his part of the discovery—exactly ten years before the printing of this book—was patriotically held by his fellow-citizens, who maintained a steady combustion of bonfires and torches for three successive nights in front of the birthplace of the distinguished explorer. That house is now a hospital, and bears the inscription:

"Ob repertam Americam sui et patriæ nominis illustratori amplificatori orbis terrarum."

Florence had soared to the highest attainable altitude in art and culture, in science and in literature, during the glorious reign of Lorenzo the Magnificent, which had been completed a few years before. "With peculiar pleasure (says Macaulay) every cultivated mind must repose on the fair, the happy, the glorious Florence, the halls which rang with the mirth of Pulci, the cell where twinkled the midnight lamp of Politian, the statues on which the young eye of Michael Angelo glared with the frenzy of a kindred inspiration, the gardens in which Lorenzo meditated some sparkling song for the May day dance of the Etrurian virgins. Alas for the beautiful city! . . . A time was at hand when all the seven vials of the Apocalypse were to be poured forth and shaken out over those pleasant countries, a time of slaughter, famine, beggary, infamy, slavery, despair." The author just quoted enumerates, however, but six vials, and to these I can show some reason for adding a seventh.

In the volume before me, written by one of the most prominent practitioners of the most cultivated country in Europe, and presenting the most striking features of the diseases which had especially impressed the mind of the author, we find that the place of honour is given to a chapter entitled "De morbo quem vulgo Gallicum vocant." He speaks of it as a new kind of disease, which, in the year of grace 1496, had suddenly invaded, not only Italy, but nearly the whole of Europe. Commencing from Spain, it first passed into Italy, then into Gaul and the other parts of the Continent. Judging from the tone of the author's remarks, he must have seen plenty of it, although but eleven years had elapsed from the introduction of the new disease into the old world. He does not explain why the dubious compliment was paid to the French nation of giving its name to the disorder, and it is at least curious that this epithet should have been popularised within a few years from the advent of the great social plague. For a couple of centuries this title of "morbus Gallicus" was much used, and a number of treatises on the venereal disease were published under that name. It even

found its way into general literature; we have Pistol of dramatic fame announcing to his audience—

> "News have I that my Nell is dead i' the spital,
> Of malady of France."

In addition to the primary lesions, the author of this volume describes, as the remoter results of infection, several varieties of squamous and pustular eruptions, ulcerations of mouth and pharynx, and severe articular pains.

Among remedial measures he recommends bleeding from the *vena media*, "*quam nigram medici vocant*," and suggests as of some slight use the employment of wet cups, or the variety of leech termed *sanguisuga*. Also the use of various emollients and alteratives. A prescription for a sudorific ointment is given:—

Resinam
Terebintinam
Argentum Vivum
Masticem
Litargirium
Cerrusam
Thymica
Massam excipiunt myrtino oleo et larido.

The "*argentum vivum*" does not, however, appear to be specially advocated.

Chap. VIII. describes the symptoms of a young woman of sixteen possessed of an evil spirit. Her paroxysms remind one forcibly of a modern case of hystero-epilepsy, as the author talks of her jumping on to and off her couch, and of her bringing the soles of her feet in contact with the nape of the neck in the interval. These spasms used to be preceded by severe pain in the lower part of the abdomen, which region became rapidly distended so as to present the appearance of that of a woman in the eighth month of pregnancy.

Chap. XXVIII. describes carefully and clearly the appearances and surgical treatment of a case of imperforate hymen. The author had recourse to crucial incision and removal of the pieces, with a completely successful result.

Chap. LXXI. gives a clearly recognisable description of senile gangrene of the toes, a lesion with which the name of Mr. Percival

Pott is usually associated in these countries. He especially dwells on the prognosis, as he declares the disease to be fatal when it occurs in this position.

Chap. LXXX. details the surgery of a calculus in the female bladder, which had become impacted at the proximal end of the urethral canal, so as to cause retention of urine. It was first fixed by a hook, and then broken in pieces by passing a blunt iron instrument along the urethra, down to the stone, and striking with a hammer. A completely satisfactory result followed the abstraction of the fragments.

Such is the tone of the medicine and surgery of this pioneer volume, which indicates a good deal of diagnostic and descriptive power, and shows a very fair amount of surgical skill and empirical knowledge of the action of drugs. It represents an immense advance on the mediæval monkish treatment, when the diagnosis was completed, and remedial measures adopted, from an examination of the excreta of the patient—sent from any distance according to the convenience of the parties interested. From those days of purely imaginative diagnosis and treatment, we have had a gradual advance to the purely materialistic practice of the surgical "Jack the Ripper" who is sometimes heard of in the present day, and who attempts the removal of all morbid products through an incision or a trephine hole. And the appearance of the volume furnishes a tangible landmark to the inquirer who is interested in the gradual progress of the healing art. It really records the practice of an earlier date than that to which the title of this note refers, as the book was not published till seven years after the death of the author, and by his surviving brother. It also forms an interesting specimen of the medical latinity of the epoch, while it can afford to take a respectable rank as an example of early printing. Accordingly, I thought that I might, for a few minutes only, ask the attention of the Medical Section to this era of our professional history, for this important memento of which I am myself indebted to Dr. Aquilla Smith, the Nestor of our profession in Dublin, who still remains, in his green and hearty old age, the highest authority amongst us on mediæval medical literature.

ANGINA PECTORIS IN THE HEART-PALSY OF ACUTE INFECTIVE DISEASES.

By JOHN WILLIAM MOORE, B.A., M.D., M.Ch., Univ. Dubl.;
Fellow and Registrar of the King and Queen's College of Physicians;
Physician to the Meath Hospital, Dublin.

[Read in the Section of Medicine, January 24, 1890.]

The object of this paper is to bring under notice a cause of "breast-pang," or angina pectoris, which seems not to have attracted that attention to which I think it is entitled. The cause to which I refer is the cardiac paralysis due to acute infective diseases, and, among them, especially to diphtheria. Within the past year three notable cases of heart-failure in infective disease have come under my observation. Of these the first and third were cases of diphtheria; the second was a case of septicæmia. The first showed no marked anginal symptoms; in the second and third these were present in an extreme degree.

Case I.—A young lady, aged twenty-three, was attacked with sore throat, of a very painful kind, on Wednesday, Jan. 9, 1889. The pain was so great as seriously to interfere with swallowing. I saw her next day, and found a membranous exudation already invading both tonsils, the mucous membranes about the throat being intensely injected and swollen; P. 110; Resp. 26; T. 103·3°. On Friday, Jan. 11 (3rd day), there were extensive diphtheritic ashy-gray foul sloughs on the inner face of both tonsils, particularly the right. The breath was fœtid. On the 12th the urine was turbid from urates, of high density (sp. grav. 1034), and with nitric acid yielded crystals of nitrate of urea, without concentration, at a temperature of 50° Fahr. On the 13th the spec. gravity of the urine rose to 1041, and there was in it a trace of albumen. The patient progressed favourably, but the albuminuria became more marked, the note on the 21st being "sp. gr. 1036—highly albuminous."

On Feb. 4 the sp. gr. had fallen to 1020; there was still much albumen in the urine, which deposited a normal quantity of epithelium and mucus, with many spherules of urate of sodium, resembling leucine spherules. A few days afterwards the quantity of albumen began to lessen, and finally the albuminuria disappeared. At no time were tube-casts detected on microscopical examination.

And now a remarkable train of paralytic phenomena unfolded themselves. On February 12th the eyesight became defective, and there was photophobia. On the 14th Mr. J. B. Story carefully examined the eyes, and pronounced the lesion to be paralysis of accommodation. On the 16th the paralysis had invaded the palate and fauces—the voice became nasal and food returned through the nostrils. This was followed by intense dysphagia, rendering the administration of nutrient enemata indispensable. On March 8th menstruation set in, accompanied by much pain, nausea, and faintness. The heart's action now became very weak, the least exertion bringing on distressing feelings of sinking, pain, palpitation, and shortness of breath. This lasted for some time, the diphtheritic paralysis meanwhile extending to the muscles of the hands, arms, and legs. Several months elapsed before the patient recovered, but in the end her health was completely restored.

In this case it will be observed that the symptoms of angina pectoris were only slightly marked.

Case II.—Miss H. R., aged twenty-one, a hospital nursing sister, was attacked with septicæmia at the beginning of April, 1889. On Good Friday, April 19, I saw her in consultation with Dr. James Hanrahan, M.K.Q.C.P., of Hollymount, Co. Mayo. She was then extremely prostrate after nearly three weeks of severe illness, including an attack of endocarditis. Under date April 23, 1889, Dr. Hanrahan wrote to me as follows:—"Poor Miss R. has had a very bad time of it since you left here. Distension and tenderness of the abdomen have increased, with accompanying killing distress and diminution of heart-power. There has also been considerable bronchial trouble. The day before yesterday the temperature fell below normal—to 97·6°—the heart became faintly audible, there was commencing lividity, with very troubled breathing, and an occasional troublesome hiccough, and, in short, collapse—altogether the case looked as hopeless an one as one could imagine. I gave 15 drops of oil of turpentine in hot whisky punch, and applied hot jars, &c. The turpentine certainly did good; and but for it I feel

confident she would have died. She had two similar attacks since, but not so desperate as the first one two days ago. Her temperature on the morning of that day was 100°. Next morning it was 99·6°, and in the evening 99·4°. This morning 98·0°, and this afternoon 99·0°; pulse 104. There has been more or less diarrhœa—3 to 5 liquid motions in the 24 hours. The kidneys have been all through acting fairly well—urine to-day is muddy, acid, and free from albumen. She has been having 5 grains of salol and 10 grains of sulphocarbolate of sodium every alternate third hour. This afternoon (5 o'clock) there is a decided change, and I think and hope for the better. The cheeks are rather flushed, the abdomen less tender and less distended. She looks much brighter and is clearer in her mind. To-day she had hot and cold sensations, and a slight rash showed on the dorsal surface of both hands and wrists and also on the legs. I look upon this rash as the result of the carbolates, so I have stopped these and ordered 4 grains of sulphate of quinine in hydrobromic acid every third hour till my next visit—the rash was very like urticaria. She has been almost all the time taking 10 drop doses of Battley's sedative, with 5 drops of tincture of digitalis, and these remedies did their duty well. She has been, and is still, having a good supply of stimulants, port wine and brandy."

On May 13, Dr. Hanrahan wrote again at length as follows:—

"Our patient, Miss H. R., has gone on as well as could be expected; but about a week ago her temperature went up to 101° and 102°; she felt very down and out of sorts; her heart became more excited and irregular in action, and the pulse ran up frequently as high as 136, though she had been lying quietly in bed; then a loud rough rasping aortic murmur became developed—evidently a second attack of endocarditis. This murmur still continues, though it is not nearly so loud as it had been. The temperature is now, I may say, normal, the heart is quieter and steadier, though far from being in a satisfactory state. Within the last five days she has had three attacks of severe pain under the left nipple, which almost took her breath away, and *alarmed* her considerably. Though I have not seen her in one of these attacks, I should say the seizures were decidedly of an anginal character. After them she is depressed and debilitated, with her heart very much excited, irregular, and weak. These symptoms and sensations pass completely away *by degrees* in the course of a few hours, and she is again bright and

lively, and feels well. The heart is now the whole trouble—the patient and I dread another attack of this kind.

"Urine fairly abundant. No albumen at any time, though deposits of lithates occur occasionally. Tongue clean, abdominal symptoms all gone, and she is inclined for food. There has been considerable dulness over the base of both lungs. There had been slight puffiness of the feet, but nothing to alarm one. She has been quite free from asthmatic attacks [to which she had been subject from childhood]—those I describe are of quite a different character, and in my judgment are decidedly anginal, notwithstanding her youth and sex, and asthmatic tendency. She is having plenty of fresh air from an open window."

A third interesting letter from Dr. Hanrahan is dated May 18, 1889, and contains these paragraphs:—

"Miss H. R. has had two very sharp attacks of angina since I wrote to you—one at 8 p.m. on Thursday and the other at 3 30 p.m. yesterday. Unfortunately, I saw neither. The nurses, as instructed, gave the nitrite of amyl, and it acted most satisfactorily, cutting the attack short in a minute or two.

"The attacks come on quite suddenly. There is no warning. She starts up, her eyes protrude with a wild intensely anxious stare, and she shrieks with agony and fear, pupils dilated, hands cold, countenance of an ashy leaden colour, lips more or less livid—she presses her hand tightly to her heart.

"This is a description of what I can gather from the nurses and her uncle and cousin, who have seen her in an attack.

"I know it leaves her very prostrate and in very low spirits, with troubled, galloping, heaving action of the heart. These symptoms pass off by degrees, and she is as bright and lively as possible.

"Her heart has been by no means acting feebly; it has been acting rather too strongly, always with a galloping, heaving action. There is no trace of a murmur at present. It is only for a short time after those attacks that her heart is feeble. Temperature normal, kidneys acting well, bowels kept regular."

I should mention that for some time the patient had been taking a digestive tonic mixture, containing hydrochloric solution of strychnin and dilute hydrochloric acid in solution of pepsin, which seemed to do her good, completely clearing the tongue and improving both appetite and digestion. On May 18th Dr. Hanrahan

wrote that she was not then getting the strychnin mixture regularly, for fear of over-stimulating the heart. He added that he was afraid to let Miss R. get up, for after having been up for a short time a few days before, she got the pain on going to bed and an attack soon followed. Dr. Hanrahan considered that complete rest for the present was essential. This opinion was fully justified by the result, for in the end Miss R. made a thorough and most satisfactory recovery. It is true that when I last examined her, on July 13, 1889, a systolic murmur was heard at the apex of the heart; but that it was of hæmic origin was at least suggested by the presence of a loud venous hum (Nonnengeräusch, or "humming-top sound") in the right internal jugular vein. In December, 1889, Miss R. was apparently quite well.

In this case the train of events was probably—(1) septicæmia, (2) endocarditis or endo-myocarditis, (3) angina pectoris.

Case III.—Walter D., aged three years, was attacked on or about Friday, August 16, 1889, with diphtheria. He contracted the disease from his elder brother, Robert, who in his turn had caught it under somewhat remarkable circumstances. The family had been in the habit of spending part of each summer at a house in one of the loveliest districts of the County Wicklow. This house and the surrounding farm-buildings and offices stood at a considerable elevation above the sea on a mountain slope. The immediate subsoil was a dry, gravelly bed, forming an island in a peaty area. In the summer of 1888 an outbreak among the cattle on the farm was described as "pleuro-pneumonia." This outbreak led to an examination of the drainage of the house and its offices. No sooner were the drains disturbed than an epidemic of sore throat occurred among the household servants and farm labourers, and that the disease was diphtheria is proved by the severity of the attack in several instances. Time went on, and the summer of 1889 found my patient's family in residence in this same house for "change of air." Sore throats once more appeared among the domestic servants, and the heads of the family at the end of a month returned to Dublin, feeling languid and out of sorts, declaring that their holiday had done them no good. The next tenants of the house were the paternal grandmother, uncles, and aunts of the children. To them the elder boy went back on a visit to the infected house, from which he had been absent only four days. In three days more he fell ill, with feverish symptoms and sore throat. He was brought home by his mother,

when I found him, on August 12, 1889, suffering from membranous sore throat, the diphtheritic nature of which was soon set at rest by his extreme pallor, great weakness, and the presence of albuminuria. On August 19th, the urine was of normal colour, slightly cloudy, and acid. Its spec. grav. was 1024. It gave a decided albuminous reaction, and darkened in a remarkable way when it was first boiled and then treated with dilute nitric acid (*urohæmatin reaction* of Dr. George Harley).* Under the microscope only a few octahedral crystals of calcium oxalate, broken up epithelium and mucus, and many vibrios were seen.

On this same day (August 19) I examined Walter D.'s urine, but it was non-albuminous, the spec. grav. being 1015. That day week, Aug. 26, Robert D.'s urine contained only a trace of albumen, and he was practically convalescent, while Walter's urine was high-coloured, acid, and highly albuminous. All this time there was much exudation on both tonsils and as far down the pillars of the fauces as could be seen. The posterior pharyngeal wall was free, although deeply injected. The temperature ranged between 99° and 102° F. Incidentally I may here mention that the father, mother, and infant sister of the two boys all were attacked, but happily recovered. Little Walter, however, "hung fire," and was clearly the subject of a succession of diphtheritic paralyses—nasal, palatal, and finally muscular and cardiac.

Believing that change of air would hasten his recovery, I ordered him to Greystones, and it was arranged that he should leave town on Tuesday, September 10. I saw him on Sunday afternoon, the 8th, and found that the slightest exertion in bed brought on distressing breathlessness and palpitation of the heart. Next morning I left Ireland for a brief holiday just at the time when a first attack of angina pectoris prevented Walter's removal to the country for the time being. In my absence he was visited and skilfully treated by my namesake, Dr. William Moore, Physician to the Queen, who allowed him to go to Greystones after a delay of a week or ten days. On my return home, I again visited the patient, but so urgent did the anginal attacks become that I placed him in charge of Dr. J. H. Armstrong, then residing at Greystones. At my request, the patient's mother favoured me on November 26, 1889, with the following graphic and touching description of the dreadful

* The Urine and its Derangements. By George Harley, M.D., F.R.S. London: J. & A. Churchill. 1872. Pages 103 and 111.

anginal attacks, or "chills," as she called them, to which the child was subject for many weeks:—

"My attention was generally drawn to the chill by Walter's restlessness, screaming, and talking wildly. It was difficult to rouse him sufficiently to take a stimulant—he seemed wandering and unconscious. I could not feel his heart beating in the least; his forehead, cheeks, ears, hands, feet, legs sometimes, and once part of his body (*i.e.*, 'the trunk') were quite cold. I tried first if rubbing and brandy would restore warmth and affect his heart—if not far gone, it has done so after some time. I have waited, though, for ten minutes without feeling any pulsation in his heart, and then given a nitro-glycerine tablet. Its effect was *always* magical. Two or three minutes after taking one I felt a slight flutter, and gradually pulsation was restored. He occasionally had three chills in the one night, but I refrained from using more than three tablets, having recourse to brandy. The chills continued at intervals for about four weeks."

The child seemed to suffer intensely in these terrible attacks, which often threatened to terminate in death. Among other remedies advised and tried were hot poultices over the heart, and wrapping the extremities in flannel wrung out of mustard and hot water.

Walter D. remained at Greystones until late in November, daily gaining health and strength. On the 27th of that month I visited him in his father's own house, and was thankful to find him getting well, although still walking with a paretic gait. The last occasion upon which I saw him was on Thursday, December 5, 1889, when he had a simple fever, which lasted only a day or so.

No one will, I think, deny that all the foregoing cases were examples of cardiac failure from zymotic disease. We have now to consider—firstly, whether I am justified in applying the term "angina pectoris" to the seizures which occurred in the second and third cases of the series; and secondly, what is the nature of the heart-lesion which seemed to lead to their development.

Cælius Aurelianus, a physician who flourished about A.D. 420, is, according to Dr. William Heberden, the only ancient writer who has noticed the complaint, which Heberden himself first called "angina pectoris," and he but slightly in the following passage:—"Erasistratus memorat paralyseos genus, et *paradoxon* appellat, quo ambulantes repente sistuntur, ut ambulare non possent, et tum rursum ambulare sinuntur." Chron. Lib. II., c. 1.

In his "Commentaries on the History and Cure of Diseases,"[a] Heberden describes the affection as follows:—

"But there is a disorder of the breast marked with strong and peculiar symptoms, considerable for the kind of danger belonging to it, and not extremely rare, which deserves to be mentioned more at length. The seat of it, and sense of strangling, and anxiety with which it is attended, may make it not improperly be called *angina pectoris.*

"They who are afflicted with it are seized while they are walking (more especially if it be up hill, and soon after eating) with a painful and most disagreeable sensation in the breast, which seems as if it would extinguish life, if it were to increase or to continue; but the moment they stand still all this uneasiness vanishes.

"In all other respects the patients are, at the beginning of this disorder, perfectly well, and in particular have no shortness of breath, from which it is totally different. . . . The pulse is, at least sometimes, not disturbed by this pain. . . .

"Such is the most usual appearance of this disease; but some varieties may be met with. Some have been seized while they were standing still, or sitting; also upon first waking out of sleep; and the pain sometimes reaches to the right arm, as well as to the left, and even down to the hands, but this is uncommon: in a very few instances the arm has at the same time been numbed and swelled. . . . I have seen nearly a hundred people under this disorder, of which number there have been three women, and one boy twelve years old. All the rest were men near or past the fiftieth year of their age."

In von Ziemssen's "Cyclopædia of the Practice of Medicine" (Vol. XIV., p. 32), under the heading of "Vaso-motor and Trophic Neuroses," Professor Eulenburg, of the University of Greifswald, defines angina pectoris as "a group of symptoms, of which the most characteristic are the following: Pain in the region of the heart, occurring in paroxysms, which usually radiates over the left side of the thorax and the left arm, more rarely over both sides and arms; the pain is associated with a peculiar sensation of anxiety

[a] Second Edition. London: T. Payne. 1803. Page 364.

and constriction, and often also with other motor, vaso-motor, and sensitive disturbances."

Dr. Richard Quain, writing to the same effect in "Quain's Dictionary of Medicine," observes that "the expression is anxious, the face is pallid, and the lips are more or less livid. The whole surface of the body is pale, cold, and covered with a clammy sweat." He adds: "An attack of angina pectoris may come on during sleep."

Dr. Stokes, in his classical treatise on "Diseases of the Heart and Aorta,"[a] says that "in the present state of knowledge we must follow Dr. Latham, in considering angina pectoris rather as a special set of symptoms than as a disease having a fixed anatomical character." He concludes "that the special group of symptoms described as angina pectoris by Heberden, Parry, Percival, and Latham, is but the occurrence, in a defined manner, of some of the symptoms connected with a weakened heart." Dr. Stokes points out that the individuals most liable to angina are those in whom we find some form of *weakened* heart, the evil effects of which will be augmented by associated organic diseases, engaging the muscular structures, endocardium, valves, coronary arteries, or the aorta itself. "These considerations," says Stokes, "lead to the doctrine long since indicated by Parry,[b] that the symptoms of angina arise from a temporary increase of weakness in an organ already weakened."

Dr. Walshe[c] observes that genuine angina pectoris is undoubtedly a very rare affection. "On the other hand," he says, "*I almost daily* meet with a form of complaint combining in a minor degree many of the characters of angina; and to this imitation of the true disease I propose to give the name of pseudo-angina." "Still," says Professor Gairdner,[d] "the fact of sudden death, superadded

[a] Dublin: Hodges and Smith. 1854. Page 482.

[b] An Inquiry into the Symptoms and Causes of the Syncope Anginosa, commonly called Angina Pectoris. 1799.

[c] Diseases of the Heart and Great Vessels. Fourth Edition. 1873. Page 208.

[d] A System of Medicine. Edited by J. Russell Reynolds, M.D., F.R.S. Vol. IV. London: Macmillan & Co. 1877. Page 535. Art., Angina Pectoris and Allied States, including certain kinds of Sudden Death.

to the evidence of certain sensations preceding death, may be considered to afford the nearest approach we have to an accurate definition of this disease."

It is true that in neither of my cases did sudden death occur, but I think it must be obvious that both the patients were during the seizures in the very Shadow of Death. Nor can it be denied that the symptoms of angina were present in both instances. It may be interesting to quote cases which appear to me to be analogous in a less or greater degree to those I have detailed.

And first, with regard to the case of Miss H. R., Dr. Stokes,[a] speaking of pericarditis, says that the pain in this affection has been observed to resemble that of angina pectoris in a remarkable degree. Thus, in a case by Andral,[b] the patient was subject to dreadful exacerbations of pain extending through the entire of the left side, accompanied by numbness of the left arm, alternating with extreme pain. On three occasions the respiration became difficult, the pulsations of the heart tumultuous, the pulse imperceptible, and the surface of an icy coldness. On the subsidence of the paroxysm the heart's action would again become regular. In this case dissection discovered abundant concretions of coagulable lymph in the pericardium, and the sac itself was distended by a large quantity of bloody fluid.

I am at a loss to know why Dr. Stokes did not at once describe such a case as one of angina pectoris in pericarditis, just as I have described the case of Miss H. R. as one of angina pectoris in endocarditis.

In the case of the little boy W. D., the seizure apparently to some extent resembled an attack of the "angina pectoris vasomotoria" described by Nothnagel in the third volume of the *Deutsches Archiv für klin. Medicin*, 1867, page 309. According to Dr. Hilton Fagge,[c] the peculiarity of this form lies in the fact that the earliest and most conspicuous symptoms of the paroxysms from

[a] Loc. cit. Page 49.

[b] Clinique Médicale. Tome I. Obs. iii. Page 15.

[c] The Principles and Practice of Medicine. Vol. II. Page 13. 1886. London: J. & A. Churchill.

which the patient suffered were coldness and pallor, with numbness and stiffness of the limbs; the palpitation, the feeling of oppression at the chest, the giddiness, the sense of impending death, being all apparently secondary and attributable to the increased efforts which the heart was called upon to make to overcome the resistance opposed to it. The attacks were also definitely traceable to external cold, and pain appears to have been a much less marked symptom than in ordinary angina. On the whole, Dr. Hilton Fagge thinks that one may take Nothnagel's cases as proving that a sudden increase of tension in the peripheral arteries, due to a cause acting upon the body from without, is capable, in some persons, of giving rise to phenomena approaching those of a paroxysm of angina pectoris.

To the recently published first volume of the "Cyclopædia of the Diseases of Children," edited by Dr. John M. Keating, of Philadelphia, Dr. J. Lewis Smith, Clinical Professor of Diseases of Children in Bellevue Hospital Medical College, New York, contributes a splendid monograph on "Diphtheria." Dr. Smith discusses the various forms of diphtheritic paralysis under the headings—1. Loss of the Tendon Reflexes; 2. Palatal Paralysis; 3. Multiple Paralysis; and 4. Cardiac Paralysis (the cardio-pulmonary paralysis of certain French writers). In illustration of the last form, he briefly narrates the clinical histories of a series of cases of sudden heart-failure, which occurred in the hospital service of one of the most trustworthy clinical teachers of the present time—M. Cadet de Gassicourt. These clinical records are as follows:—

Case I.—A child of two years entered Cadet de Gassicourt's *service* on January 3, with diphtheritic pharyngitis of ten days' continuance. The tonsils were large, still covered with pseudo-membrane, and the submaxillary glands were also enlarged. He had no laryngeal symptoms, and his urine was without albumen. On the following day the velum and pharyngeal muscles were slightly paralysed, the speech nasal, and deglutition moderately embarrassed. He was quiet during the night of January 4 and on the morning of the 5th, but at 10 a.m. he became chilly, his face and extremities feebly cyanotic, and slight dyspnœa and dilatation of the alæ nasi were observed. His pulse, at first abnormally slow,

became rapid; he was agitated, uttered loud screams of distress, and fell back cyanotic and dead. The death-struggle did not occupy more than one minute.

CASE II.—Another infant, also two years of age, entered the same service, having diphtheritic pharyngitis of two days' continuance. The fauces presented the usual red appearance, the tonsils were swollen and covered with a thick exudate, but there was no albuminuria or croupiness. Two days later the pseudo-membrane had diminished, but the velum palati was paralysed. On the following day the general appearance was satisfactory, and the pseudo-membrane had still further diminished. At 8 p.m. the infant was suddenly seized with vomiting, accompanied with great dyspnœa, rapid pulse (160), and a cyanotic hue of the face and extremities. He was restless, and uttered cries of distress. Two hours later he screamed loudly, raised himself in bed, and fell back dead.

CASE III.—A child of five years was admitted with diphtheritic pharyngitis of two days' continuance, having enlarged tonsils covered with pseudo-membrane, and enlarged cervical glands, but without cough or albuminuria. Seven days later—the ninth of the disease—the pseudo-membrane had disappeared, but the velum palati was paralysed. On the following day there was little change, except occasional vomiting, but the general state was good, and sleep tranquil. At seven a.m. on the following day—the eleventh of the disease—after a calm night, the child uttered two or three cries, the pulse became rapid, the respiration embarrassed, the features, extremities, and finally the entire surface, cyanotic, and at 8 a.m. death occurred quietly.

The similarity of these three cases is apparent. Paralysis of the velum and palate had continued in the first case eighteen hours, in the second case thirty-six hours, and in the third case forty-eight hours, when suddenly the heart and lungs were greatly embarrassed in their functions, and death occurred within one hour from the commencement of the severe symptoms. The agitation, repeated cries of distress, and the shrill cry that preceded death, indicated extreme suffering.

Severe pain, præcordial, epigastric, or abdominal, is present in some if not in most of these cases of sudden heart-failure.

It was probably experienced by these three patients, who were too young to express clearly their subjective symptoms.

It is interesting to observe the great similarity to these three cases presented by the seizures, described as "cardiac syncope," to which the infant king of Spain was subject towards the close of the attack of influenza, from which he suffered at the beginning of this year [1890]. Nor is his case devoid of interest from an ætiological standpoint, when we remember that the poison of influenza seems to have a special affinity for the central nervous system, and particularly that portion of it which presides over the innervation of the lungs, the stomach, and the heart.

Gombault made a minute microscopic examination of the affected organs in Cadet de Gassicourt's three fatal cases after the tissues had been properly hardened by chemical agents. The points of special interest in the microscopic examination were the apparently healthy and normal state of the pneumogastrics and myocardium in the one case in which they were examined, and of the medulla oblongata in the three cases, while the gray matter of the spinal cord, which has no immediate nerve connection with the heart, showed marked degenerative changes.

Dr. Lewis Smith views with favour the theory that weakening of the heart's action in diphtheria, with sudden death as a consequence, should, perhaps, be attributed to granulo-fatty degeneration in the muscular fibres of the heart, consequent upon a prolonged and severe diphtheritic attack. But to him the theory of deficient innervation or a true cardiac paralysis appears most tenable under the circumstances. He sums up the situation in these words:—"The action of the heart may be feeble from granulo-fatty degeneration of its muscular fibres, or from anæmia or general weakness; but sudden and unexpected death from heart-failure is commonly due to paralysis of this organ."

Although Gombault failed to discover any alteration in the myocardium in the one case of Cadet de Gassicourt's series, in which he examined that structure microscopically, the weight of scientific medical opinion inclines to the proposition that in such cases as those described there is a profound lesion of the muscular

tissue of the heart. At the close of the first volume of his Lectures on Children's Diseases,[a] Professor E. Henoch, of the University of Berlin, writing on "Myocarditis," says—"When I come to consider the infectious diseases, I shall return to the fatty albuminous degeneration of the heart muscle which occurs pretty often after acute infectious (? infective) disease, especially scarlet fever, diphtheria, and typhoid, and clinically gives rise to no symptoms, except, perhaps, those of cardiac debility." (Page 491).

In the second volume of the Cyclopædia of the Diseases of Children there is a short, but interesting, article on "Acute Parenchymatous Myocarditis," from the pen of Dr. J. Mitchell Bruce, Physician and Lecturer on Therapeutics at the Charing Cross Hospital, London. This article seems to me to throw much light on the ætiology of the anginal attacks of acute febrile disorders. Under the names of "acute parenchymatous degeneration," "albuminous degeneration," "febrile softening of the heart," "infectious myocarditis," Dr. Bruce says that from time to time has been described a kind of acute change in the muscular tissue of the heart, which occurs in acute febrile and infective diseases. The opinions of pathologists as to the nature of this disease have long been, and are still, conflicting, some maintaining that it is truly inflammatory, others that it is degenerative only.

"Parenchymatous myocarditis" is the result of acute febrile and infective processes, such as scarlatina, diphtheria, variola, typhus, typhoid, and relapsing fevers, septicæmia and pyæmia, more rarely measles. The condition may be set up during the later, as well as in the earlier stages of these diseases, or even during convalescence. In it the heart is sometimes distinctly dilated; the myocardium is of a dirty grayish red or grayish yellow colour, with occasional extravasations; its consistence is soft; its substance is lax, flabby, and friable. Thrombi may be found in the ventricles. Microscopically, the muscular fibres are swollen, their striation is more or less lost and replaced by granular (albuminous) and fatty molecules; occasionally they undergo waxy degeneration

[a] Translated for the New Sydenham Society, from the fourth German edition (1889), by John Thomson, M.B., F.R.C.P. Edin.

(Zenker). Along with these evidences of degeneration, there are found certain appearances which suggest *regeneration*. Lastly, the blood-vessels are congested and the seat of thrombosis, with obliterative endarteriitis of the arterioles.

Dr. Bruce points out that the pathological connection between this acute parenchymatous change and its causes is still unsettled. It may be the result of the specific action of the several poisons, or of the pyrexia, or of both, on the protoplasm. It is closely related to fatty degeneration of the heart—indeed, if the destructive part of the process be in excess, it rapidly proceeds to fatty degeneration, which then covers, or takes the place, of the other changes.

As regards the symptoms, *cardiac failure* is the chief evidence of this condition of the myocardium. "Either slowly or suddenly," says Dr. Bruce, "a child suffering, for instance, from diphtheria, falls into a condition of collapse. The pulse fails at the wrist, becoming feeble, small, irregular, and either very frequent or remarkably infrequent. The countenance is pallid, with some lividity and expression of apathy; not greatly distressed, with pain and dyspnœa, as in ordinary acute myocarditis. The cardiac impulse and the first sound become weaker, and may disappear. Galloping rhythm, or a systolic murmur, is sometimes developed. The extremities are cold, the skin is bathed in sweat. The urine contains albumen. Death occurs in most cases, either slowly, with hypostatic visceral congestions, increasing dyspnœa, and asphyxia, or suddenly by cardiac arrest. Recovery is, however, possible."

Oertel, of Munich, writing on "Diphtheria" in von Ziemssen's "Cyclopædia of the Practice of Medicine,"[a] observes that "the muscular substance of the heart itself, in cases in which the disease has spread principally in the air-passages, and death by suffocation has ensued, appears entirely unaltered, as well in colour and texture as in its histological elements. But if, on the other hand, the diphtheritic process has attacked the whole organism, and the patients succumb to general poisoning and septicæmia, then the muscles of the heart are more or less broken by extravasations of

[a] Vol. I. Page 646.

blood, are friable, and show in places accumulations of cells and nuclei between their fibres. When the general disease lasts long, and is very intense, and especially in cases in which death is caused suddenly by paralysis of the heart, the muscle appears pale, soft, friable, broken by extravasations of blood, and on microscopical examination most of its fibres are seen to be already in an advanced state of fatty degeneration."

To sum up:—

1. The group of symptoms described as "angina pectoris" may show themselves in the latter stages of any of the acute infective diseases, particularly septicæmia, or pyæmia, or diphtheria.

2. The anginal attacks of acute infective disease often answer the definition given by Dr. Byrom Bramwell of true angina pectoris—namely, "a neurotic affection characterised by paroxysms of intense pain in the region of the heart, and a terrible sensation of impending death . . . the affection is in many cases associated with organic disease of the heart and the root of the aorta, and in its typical and severe forms is apt to prove suddenly fatal."[a] Be it noted that this author, while drawing a clinical distinction between true angina pectoris—rarely met with before the age of forty—and the pseudo-angina, or attacks of cardiac pain to which young persons are liable, is careful to state "that the two forms run one into the other, and that it is sometimes difficult or impossible to separate them at the bedside."

3. These seizures of angina in infective disease appear to arise in the following ways:—

(α) Deficient innervation of the heart, or a true cardiac paralysis or heart-palsy.

(β) A granulo-fatty degeneration of the heart-muscle, the result of an acute parenchymatous myocarditis.

(γ) A sudden increase of tension in the peripheral arteries, due to a cause acting upon the body from without, reacting upon an already weakened heart—the *angina pectoris vaso-motoria* of Nothnagel.

[a] Diseases of the Heart and Thoracic Aorta. By Byrom Bramwell, M.D., F.R.C.P.E. Edinburgh: Young J. Pentland. 1884. Page 672.

4. Any one of these causes may operate singly in a given case; or two or more of them may be combined so as to determine an anginal attack.

5. Treatment is often attended by the happiest results in the angina of heart failure from infective diseases—the most useful therapeutical measures being:—

(1.) In (α), the administration of alcoholic and diffusible stimulants; the application of hot poultices over the heart, and of mustard epithems to the extremities.

(2.) In (β), besides the foregoing, the administration of heart tonics and stimulants, such as nux vomica and strychnin, arsenic, digitalis, convallaria, and strophanthus.

(3.) In (γ), the administration of the nitrites—spirit of nitrous ether, nitrite of amyl, nitro-glycerine, as well as iodide of ethyl, and such like remedies.

A REMARKABLE CASE OF INTESTINAL OBSTRUCTION.

By WALLACE BEATTY, M.D.;
Senior Assistant-Physician to the Adelaide Hospital.

[Read in the Section of Medicine, January 24, 1890.]

WHEN a case of intestinal obstruction comes before one, it is often extremely difficult, at times impossible, to arrive at certainty in the diagnosis of the nature of the obstruction, as the group of symptoms which is present may not point with certainty to one special form. The case I am about to relate is one of these obscure cases and, even when it terminated—as it did fortunately—in recovery, and one could look back calmly upon its symptoms and course, it was scarcely possible to do more than conjecture as to its nature:—

CASE.—H. S., a boy aged 10, came under my care on January 16th last. The history of his case is as follows:—He had always been a healthy boy, and, except for an occasional attack of diarrhœa of only a few hours duration, he had never any illness. His bowels usually acted twice every day when he was in perfect health.

The present illness began on the night of Sunday, January 13th; during the day he was quite well, but at night he was attacked by pain in the abdomen, rather severe but not agonising. On Monday morning, January 14th, he vomited. During that day and the next he had diarrhœa and some vomiting, and the pain continued.

When I saw him on the morning of the 16th (*i.e.*, the 4th day) he complained of abdominal pain, felt chiefly but not strictly localised in the right iliac fossa; the abdomen was a little distended and tense. There was some tenderness on pressure in the right iliac fossa. His pulse was 100; temperature was 99°; his tongue was thinly furred; there had been no hæmorrhage from the bowels.

I was uncertain what was wrong with him; he did not look very ill. The suddenness of the attack made me think of intestinal obstruction, and the presence of abdominal pain and tenderness in the right iliac fossa were in favour of typhlitis.

As abdominal pain and tenderness were the chief symptoms, I put him on 4 minims of Battley every 4 hours. In the evening his temperature was 100°; his pulse 104. The opium had given him considerable relief; the vomiting had ceased.

The next day, January 17th (*i.e.*, 5th day), there was still some distension of the abdomen and some diarrhœa, but he was apparently better—he had scarcely any pain, no vomiting; his temperature was normal both in the morning and in the evening, and his pulse was between 80 and 90. Though I gave a guarded opinion I hoped that the symptoms were subsiding. The treatment was continuance of the Battley, 1 grain of calomel guarded by 2 grains of Dover's powder. He was ordered whey and Brand's essence.

On January 18th (*i.e.*, 6th day) there was still some distension of the abdomen, the tongue was furred, he had diarrhœa, the motions were liquid and were somewhat like those seen in typhoid fever—viz., pea-soupy. The morning temperature was 99·6°; the evening temperature was 100°. The pulse both in the morning and in the evening was 84. As there was still tenderness in the right iliac fossa I applied there two leeches.

On January 19th (*i.e.*, 7th day) his condition was much the same as it was on the previous day. His abdomen was still tense; there was tenderness in the right iliac fossa; the temperature was raised, reaching 101° in the evening; the bowels were still acting too freely, having moved 3 times during the previous night.

I continued the opium and applied cold compresses to the right iliac fossa.

On January 20th (*i.e.*, 8th day) the abdomen was still somewhat swollen and tense. There was some distension in epigastric region—any tenderness there was was very slight. The morning temperature was 99·4°; the evening temperature was 100°. The morning pulse was 100; the evening pulse was 84.

On January 21st (*i.e.*, 9th day) he had a large lumpy motion—the lumps were not hard. The abdomen was softer in the morning, but in the evening became again rather distended. The morning temperature was 98·6; the evening temperature was 100°. The pulse was 80 in the morning, 76 in the evening.

My note taken on January 26 (*i.e.*, 14th day) is as follows:—

"since last note on January 21st his condition has been improving. He has for the last few days been looking better and said he felt quite well; still his tongue though it began to clean from the tip a few days ago, is now furred and the tip is again coated. His temperature is still slightly raised towards the evening; and his belly is still somewhat tense, though less so than before.

Yesterday, Jan., 25th (*i.e.*, 13th day), I detected some hardness in the right iliac fossa—no distinct well-defined tumour. A few days ago I gave an enema of castor oil and water and repeated it the next day; no fæcal lumps of any size came away. Yesterday I had a firoin-sized blister applied to the right iliac fossa. At midday his temperature was 100·2°." I thought the disease, probably, was typhlitis.

On January 27th (*i.e.*, 15th day) his abdomen was much flatter, but there was still some tension—much less, however, than before. The bowels had moved during the night—two or three long cylindrical masses came away, perfectly natural in form, but a little pale in colour. In the morning a motion was passed containing small curds (*i.e.*, some milk undigested). The morning temperature was 99·4°.

On January 28th (*i.e.*, 16th day) Dr. Head saw him with me. He considered the case an obscure one, inclined to the view of there being some inflammatory mischief in the region of the cæcum.

On January 31st (*i.e.*, 19th day) my note on that day was—"since the 27th no change occurred until to-day. Every day I hoped that the belly would lose its tympany, and that the moderate fever would abate. The temperature varied from 99·5–100° in the morning to about 101° at night for the last two or three days. On this morning a change in the symptoms occurred; he began to vomit and got very severe pain in the abdomen. During the day the bowels moved twice—on each occasion a normal cylindrical motion was passed. The pain became so severe to-day that I gave 8 minims of Battley at each dose instead of 4; I also gave 2 grains of calomel with ½ grain pulv. opii. at bedtime."

On February 1st (*i.e.*, 20th day) he vomited everything, even water. There was no action from the bowels. At 3 p.m. Dr. Head saw him with me. The abdomen was distended, chiefly in the region of the umbilicus. Pain came on in severe paroxysms; during these paroxysms the peristalsis of the intestines was visible on the surface of the abdomen. The boy looked very ill; a marked change had occurred in his aspect compared with what it was two

days before. It was clear he had intestinal obstruction of some kind. The treatment adopted was—all food by the mouth was stopped; he was given nutritive enemata. To relieve the pain he was given $\frac{1}{7}$th grain morphia hypodermically, with a little atropia. (The hypodermic solution I used on this day and subsequently contained in each 5 minims $\frac{1}{4}$ grain sulphate of morphia, $\frac{1}{100}$ grain sulphate of atropia.)

In the evening Sir William Stokes saw the boy with me. We considered whether the intestinal obstruction could be relieved by operation; we decided in the negative, because we believed that the cause of the obstruction was paralysis. We gave him 2 grains of calomel with $\frac{1}{2}$ grain pulv. opii. From this time until the termination of the illness I had the valuable and kind assistance each day of Sir William Stokes.

On February 5th (*i.e.*, 24th day), the note taken was—"The history since February 1st is briefly this:—He has been taking by the mouth since February 2nd peptonised milk in wineglassfuls. Ten minims of tincture of belladonna every four hours, and nutrient suppositories. Twice or three times in the day he has been given morphia hypodermically, and laudanum on lint under a poultice was kept constantly on the abdomen. The abdomen until to-day was not universally swollen, the swelling being situated chiefly in the region of the umbilicus. To-day the abdomen is very much distended. After purgative enemata some brown fluid came away a few times from the bowels—not true fæces. Yesterday (Feb. 4) he passed a good deal of wind per rectum. When the effect of the morphia passed off the pain and inclination to sickness, and sometimes vomiting of green bile, occurred. Treatment—The belladonna by the mouth was stopped, and four grains of calomel were given February 6th (*i.e.*, 25th day). No motion followed the calomel. The abdomen was very greatly and universally distended. The superficial abdominal veins were prominent. He did not pass much water during the previous twenty-four hours; urine was pale; he vomited green bile during the previous night; tongue was coated; the temperature during the previous few days was normal or subnormal; the pulse was variable, occasionally its frequency was 56 or 60, more often it was between 70 and 80, and sometimes it reached 90; an injection was given with the long tube; some solid and fluid fæces came away, but in small quantity; the solid matter was soft; after the injection there was no appreciable change in the contour of the abdomen, it was still very

tightly distended; during the day morphia had to be given hypodermically three times; a second purgative enema was given in the evening; this was not successful: the urine that day was tested for indican with negative result; the colour of the urine was pale yellow."

On February 7th (*i.e.*, 26th day), the belly was still very tightly distended. The temperature was in the morning 97·8°, and pulse was 88. He was given by Sir William Stokes a large enema of ox gall, soap, and water. The long tube was used, and was passed up about 18 inches. The enema brought away nothing.

From February 7th to February 16th his condition was as follows:—The abdomen became, if possible, more and more swollen each day. The superficial abdominal veins were much distended, and the left superficial epigastric artery pulsated strongly. The temperature was subnormal all along; the pulse was variable; when the vomiting ceased for a time the pulse improved; vomiting of green bile occurred on and off; during Feb. 14, 15, and 16 there was no vomiting. His sufferings were very great each time the effect of the morphia hypodermics had passed off; the pain increased each day in severity, and was, as before, paroxysmal, and attended with visible peristalsis. A little wind passed per rectum on the 13th Feb., after a large injection had been given, and a little solid fæces; in spite of this the abdomen remained enormously distended. On February 14 electricity was tried without any result. On February 15th, after a large purgative injection, a few fæcal particles, but no wind, came away. On February 14th we observed superficial marginal ulceration of the lower part of the left cornea, with some whitish infiltration, and a somewhat similar, though less marked, condition in the right eye; for some days previously his eyes had been bloodshot. On February 15th Mr. Swanzy saw the boy in consultation with Sir William Stokes and me. His opinion was that the marginal ulceration in each cornea was due to the boy lying with his eyes half open, and never closing them completely, even when asleep. He recommended that they should be covered with boric acid ointment on lint. He had been keeping down a fair amount of nourishment during Feb. 14, 15, and 16—viz., about one pint of peptonised milk, yolks of three eggs, a little chicken broth, and beef tea; in addition he was still getting nutrient suppositories. The boy was in a sad, hideously emaciated, condition. It was pitiful to see the haggard face, sunken cheeks, the bloodshot eyes, and the look of extreme suffer-

ing, when he was not under the influence of morphia. It was marvellous that he had the strength to get out of bed each time he wanted to pass water, or to attempt to evacuate his bowels.

On February 16th (*i.e.*, 35th day), he required 1¼ grains of morphia hypodermically in the twenty-four hours. The pulse on this day was about 112.

His condition from February 16th up to February 23rd was—the pulse was becoming more frequent and weaker; on the night of February 22 it was almost thready; I thought the boy was sinking; the next day the pulse improved. On two occasions, each time after a large injection (containing turpentine, castor oil, and ox gall), a very little wind and a few fæcal particles came away. No difference, however, was observed in the distension of the abdomen. He still could get up strongly to pass water; sometimes he could not succeed in passing it.

On February 23rd (*i.e.*, 42nd day), the distension of the abdomen was so extreme, and the distress occasioned thereby so great, that Sir William Stokes passed a trochar and canula into a projecting coil of the bowel, a little above the umbilicus. A good deal of gas escaped and the belly became softer. After the aspiration he went to sleep for a time.

On February 24th (43rd day) the pulse was decidedly stronger; the abdomen was not so extremely tense as it had been before the aspiration. Sir William Stokes examined the rectum with his finger; the bowels were felt pressing on the rectum. During the night 2 grains of morphia were given hypodermically; during the day 1½ grains—*i.e.*, 3½ grains in the twenty-four hours.

On February 28th (*i.e.*, 47th day), as percussion revealed dulness at the left side of the abdomen, a hypodermic needle was introduced at the upper part of the left iliac fossa; it evidently entered the bowel, as fluid fæces were withdrawn. We thought that possibly the dull note might indicate a collection of matter, but the puncture showed that it was due to intestines loaded with fluid fæces.

On March 2nd (*i.e.*, 49th day), the note taken was—"During the night there was much retching; the pulse was very thready; I wonder he did not die; the abdomen is very tense."

Two aspirations were made by Sir William Stokes on that day. Much gas escaped, and considerable temporary relief was experienced.

March 3rd (*i.e.*, 50th day) was a very eventful day. In the morning Sir William Stokes and I met. Hope, which each day

had been becoming fainter and fainter, we had to abandon. The boy was apparently dying; his hands were cold; his pupils dilated; he was quite apathetic; did not answer questions; breathing was very slow; at one time only four respirations in a minute—in fact, he looked like death. To do anything—*e.g.*, to make one more trial with an enema, was out of the question; it appeared as if, at any moment, he would breathe his last. At 1 30 p.m. I saw him again. He still lived, but there was no change. I wondered that he still clung to life, but felt sure that before night the final breath would come. At 5 30 p.m. a car was driven hurriedly to my door; the boy's father jumped off, exclaiming, "He has had a motion!" In a few minutes I saw him. Wonderful to relate, he had passed a copious fæculent motion, and his pulse had improved greatly. Sir William Stokes was summoned. We gave an enema of turpentine and oil, and fæculent fluid came away; there were a few soft fæcal lumps; the pulse was 100.

During the night of March 3rd he passed an immense quantity of semi-fluid fæces—in all more than seven pints (seven pints were measured). At 4 a.m. on March 4th he was given his last morphia hypodermic. Sir William Stokes and I agreed to stop the morphia. During March 4th several motions were passed; the belly had become soft; the pulse was about 90 to 100; his temperature was normal. In the evening he became delirious; one time he fancied he was going to Ball's-bridge; at another that he was shooting his mother.

On March 5th he would not speak; he grunted like a fog-horn! in the evening he spoke single words—*e.g.*, "hang," "hurt," "more" (this last when he had just been given beef tea); he bit his mother when she stooped to kiss him; at night there was some diarrhœa; the abdomen was soft; nothing abnormal could be discovered; no tumour; the pulse was regular and of good volume. Treatment—Peptonised milk, beef tea, Valentine's meat juice, a little champagne. Though his pulse was good, he was physically very weak, could scarcely move a limb.

My note on March 12th was—"He has been getting on well; his bowels act freely, and he takes his nourishment well; his diet consists of peptonised milk, eggs, coffee, and milk, gruel, and a little fish for the past two days. Tongue is nearly clean, but he is still delirious, and every now and then there is visible peristalsis of the intestines."

The delirium ceased on March 15th. During the previous week he had gained in flesh, and was each day looking better. At 8 p.m.

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on March 16th his condition became again alarming. He began to suffer from severe abdominal pain, coming on in paroxysms, and there was visible peristalsis; this pain lasted for two days severely; while the severe pain continued there was no motion passed; an enema given on March 18th acted; after this the severe pain ceased, and in a few days the pain entirely went away.

He made an excellent recovery, and got fat and strong. Since this illness he has enjoyed excellent health, except on one occasion, in August last, when he had an attack of diarrhœa and vomiting; the attack began at midnight. The day before he had eaten for breakfast two eggs and lobster, and during the day had fatigued himself; he ate a lot of chocolate during the day. I saw him in the afternoon of the following day; his temperature was normal; his pulse quiet, his tongue clean, and there was nothing abdominal discovered; in a couple of days he was quite well.

In reviewing this case it will conduce to a clearer understanding of all its bearings if we consider three periods:—1. The period from the boy's birth to the commencement of his illness; 2. The illness itself; 3. The period from the illness up to the present time.

With regard to the first period there is not much to record. He had enjoyed excellent health. The only points of importance are—1. Occasionally he had an attack of diarrhœa; 2. His bowels were almost always moved twice each day when in perfect health. So a rather too active intestinal peristalsis is the only noteworthy point in his past history.

With regard to the second period—viz., the illness itself, it may be divided into two stages—the first stage from January 13 to January 31 (*i.e.*, having a duration of 19 days); the second stage from January 31 to March 3rd (*i.e.*, having a duration of 32 days).

The symptoms during the first stage may be summarised thus—

A sudden commencement—the first symptom being abdominal pain, followed by diarrhœa, and in about twelve hours by vomiting. The symptoms during the first four days were not very urgent, as no medical aid was sought until the fourth day. His condition then was abdominal pain of moderate severity, tenderness on pressure in the right iliac fossa, some tension of the abdominal walls, a temperature of 99°, a rather weak pulse, and a thinly furred tongue.

From the 4th day till the 19th—*i.e.*, until the termination of the first stage, his condition was—

1. Some diarrhœa; motions were sometimes liquid, sometimes formed, never scybalous.

2. Some fulness and tension of the abdominal walls; this was not at all great, and seemed to lessen each day.

3. Slight tenderness in the right iliac fossa.

4. Moderate fever, the highest temperature reached being 101.°

5. A small, indistinctly felt tumour in the right iliac fossa, present at the end of this stage; and—

6. No vomiting. Most of this time the boy felt very well and hungry, and was disappointed at being kept on low diet.

The treatment during this stage was a restricted liquid diet, a few drops of Battley's solution every three or four hours, leeches and wet compresses to the right iliac fossa.

The symptoms during the second stage were—a sudden change in the boy's condition, this change consisting in paroxysms of severe abdominal pain, vomiting, distension of the abdomen, and obstinate constipation. From the commencement of this stage until it terminated the symptoms were briefly as follows:—

1. Rapidly increasing distension of the abdomen, commencing in the umbilical region, subsequently involving the whole abdomen.

2. Paroxysms of abdominal pain, becoming each day more and more agonising, and only subdued by repeated hypodermics of morphia.

3. Vomiting frequent, but fluctuating, not constant, bilious, only once or twice fæculent.

4. Obstruction of the bowels almost complete. On only a few occasions some wind and a scanty amount of fæces came away, without giving any relief.

5. Rapidly-increasing emaciation, which became finally extreme.

6. A normal or subnormal temperature.

7. A pulse of fair volume during the greater part of the time, and of moderate frequency—*i.e.*, under 100; until towards the termination of this stage, it was never extremely rapid.

8. At times difficulty in micturition, and more or less tenesmus.

9. A sudden relief to the obstruction, occurring when the boy was almost dead.

With regard to the third period (*i.e.*, from the termination of the illness until the present), there are two events to record—the attack of paroxysmal abdominal pain which occurred during convalescence from his illness, and the attack of diarrhœa and vomiting which occurred some months after, and lasted about two days.

On reviewing the case, I think the diagnosis rests between—

(*a*). An intussusception;

(*b*). Typhlitis leading to obstruction by producing local paralysis.

The question of intussusception is one deserving of some consideration. There are undoubtedly some points in the case which favour this diagnosis—

1. The suddenness of the onset.

2. The first symptom being sudden pain.

3. The presence of diarrhœa during the first part of the illness which in intestinal obstruction is suggestive of intussusception.

4. The moderate distension of the abdomen for several days.

5. The fact that an indistinct tumour was felt in the right iliac fossa for a couple of days.

6. The change in the symptoms on the 19th day may have been due to an increase in the invagination leading to obstruction.

7. The sudden relief may have been due to spontaneous reduction of the invaginated bowel, the other method of spontaneous cure being excluded by the fact that no portion of gangrenous intestine was subsequently discharged.

The chief difficulties which lie in the way of the acceptance of this diagnosis are:—

1. The early fever. Still in some cases of intussusception fever has been noted. Treves, in his excellent Manual on Intestinal Obstruction, when speaking of the temperature in intussusception, says:—"It will be below normal in cases associated with shock. In the majority of the cases, and especially in such as are subacute, it is normal, or a little above normal. It is important" (he continues) "to recognise the fact that there may be a rise of temperature in intussusception apart from any evidence of local peritonitis. As

a good illustration of this may be cited a case recorded by Dr. Eastes. It concerned a little girl aged eleven. On the seventh and eighth day of the symptoms the temperature reached 101·3.° On the evening of the eighth day the invagination was reduced by means of forced enemata. On the ninth day the temperature was 97·6°.

2. The early local tenderness in the right iliac fossa.

3. The absence of bloody stools.

4. The long duration of the illness, which would be greatly against the possibility of spontaneous reduction, as it would be very unlikely that adhesions would not have formed precluding the possibility of such reduction.

How is the attack of pain with visible peristalsis, which occurred during convalescence, to be explained if we assume that the illness was due to intussusception? Perhaps it was due to a fresh invagination which reduced itself, or perhaps to a temporary obstruction due to a weakened condition of that part of the bowel which had been invaginated.

The question of typhlitis remains to be considered. During the first stage of the illness I believed that the case was one of typhlitis, on account of the early tenderness on pressure in the right iliac fossa, the abdominal tension, the fever, and the presence, after some days, of a small tumour in the situation of the cæcum. The sudden onset is quite compatible with typhlitis, as it usually begins suddenly.

The sudden intestinal obstruction which occurred on the 19th day would be accounted for by paralysis of the cæcum, due to the inflammation. This is, I think, Sir William Stokes' view; and, taking all the symptoms into consideration, it seems to me to be the true explanation of the case. But there are, no doubt, some difficulties in the way of accepting this view.

1. Why was diarrhœa present during the early part of the illness? Constipation is the rule in typhlitis.

2. How did the obstruction give way? Perhaps it was that on the approach of death a general relaxation of the intestines took place, and so the accumulated contents of the cæcum made their way out.

3. If the typhlitis was severe enough to produce paralysis of the cæcum, is it not strange that the inflammation did not spread to the

cellular tissue behind the cæcum and lead to perityphlitis and abscess? There was no evidence of suppuration either during or after the illness.

Before concluding my paper I should like to allude to the marginal ulceration of the cornea which took place during the latter part of the illness. When Mr. Swanzy saw the boy the ulceration was present only at the lower part of each cornea. His explanation was that the ulceration was due to constant exposure, as the eyes were never completely closed, even during sleep, and so the lower part of each cornea was uncovered. Some days after Mr. Swanzy had seen the boy a marginal ulcer occurred at the upper part of one cornea. How is this to be explained? Could not the ulceration have been due, as Sir William Stokes and I thought at first, to the miserable state of malnutrition in which the boy was?[a]

In conclusion, I only wish to say that I have ventured to occupy a very considerable time this evening in relating this case, because I think it is one that is remarkable in many ways:—The early fever; then the sudden cessation of fever; the early diarrhœa; the sudden constipation; the long duration of the case; the sudden relief when the boy was at the gate of death; the mental clearness before relief came; the delirium afterwards; the wonderful strength the boy showed in getting up while the obstruction lasted; the extreme physical weakness afterwards. This may in part have been due to the sudden withdrawal of the morphia.

Lastly, when observers of such long experience as Dr. Head and Sir William Stokes agree in considering my patient's case as one of the most remarkable cases they have ever seen, I may be pardoned for bringing it at such great length before the Academy.

[a] "As I did not see the ulcer Dr. Beatty mentions as having formed at the upper margin of one cornea, I am not in a position to offer an explanation for it. It may have been a phlyctenular ulcer. That the affection of the lower part of the cornea which I did see was mainly due to the non-closure of the lids, even during sleep, I still think. In consequence of this non-closure the exposed parts of the corneal epithelium became dry and disorganised, and came away, and in this way the ulceration occurred. The fact that protection of the cornea with an antiseptic dressing was sufficient to arrest and cure the ulceration is proof of the correctness of this view. The cornea of a person in perfect health, similarly exposed, would likewise become ulcerated.—H. R. Swanzy."

THE INFLUENZA EPIDEMIC OF 1889-90, AS OBSERVED IN DUBLIN.

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[Read in the Section of Medicine, February 28, 1890.]

My observations on the epidemic of influenza of 1889-90, in Dublin, extend over a period of nearly three months—from early in last December up to the end of February. The two earliest cases which came under my notice dated from Thursday and Friday, December 5 and 6, 1889, respectively. The outbreak was at its height in the first half of January. Towards the close of that month its prevalence waned quickly; but in the middle of February there was a recrudescence of the epidemic.

In this communication I propose—first, to consider the effect produced on the public health and on the bills of mortality in Dublin by the epidemic; and, secondly, to describe the impression made upon me as to the origin, nature, and course of the disease.

PART I.—STATISTICAL.

In considering the effect of the influenza on the public health we are, unfortunately, at a serious disadvantage in having no system of registration of disease. But I take it that few will be found to deny that sickliness in Dublin increased by leaps and bounds in the closing days of December, 1889, and that the following January was one of the sickliest ever experienced within living memory. The whole "Epidemic Constitution"—to use Sydenham's classical phrase—was changed for the worse; the power of resisting disease was lessened; and extreme languor and prostration passed over

the population like a pandemic, as Inspector-General Robert Lawson would say.

As to the influence of the late epidemic on the death-rate of Dublin, we have more direct and precise information in the Registrar-General's weekly returns of births and deaths in Dublin and its suburbs. We may, indeed, admit as self-evident that existing statistics of the "Medical Cause of Death," are far from perfect, having regard to the frequency of a wrong diagnosis, the confusion between "primary" and "secondary" causes of death, and the absence, in the vast majority of cases, of the corroborative evidence or otherwise, which a *post mortem* examination, conducted by an expert, would afford. Nevertheless, such statistics have a certain value, more especially when we compare the returns of one period with those of another and corresponding period, or with average figures deduced from observations extending over a long series of years.

In Table I. I have included some very interesting and instructive facts bearing upon the epidemic of influenza in the Dublin Registration District, embracing a population (estimated to the middle of 1890) of 353,082 souls.

Column 1 gives the weekly mean temperatures of the epidemic period of 1889–90; column 2 gives the average weekly mean temperatures in the corresponding periods of the 20 years, 1861–80. It will be observed that, while the mean temperature of the first two weeks of the period was about equal to the average, a remarkable excess of temperature afterwards set in, lasting for at least five weeks, and culminating in the second and third weeks of the new year, the mean temperatures of which were no less than 7·5° and 7·9° respectively above the average. Now, if one fact has been established in relation to the winter death-rate in Dublin, it is that the deaths from all causes, and, particularly, from disease of the respiratory organs, such as bronchitis and pneumonia, vary in number inversely with the temperature. If the thermometer is high in winter, the death-rate is moderate or low; if the thermometer is low, the death-rate is high.

TABLE I.—*Showing the Mean Temperature in Dublin City, and the Deaths from All and from Certain Specified Causes in the Dublin Registration District, during the last Four and first Seven Weeks of the Year, in* 1889–90, *and on the Average.*

No. of Week	Mean Temperature		Deaths from All Causes		Deaths from Zymotics		Deaths from Bronchitis		Deaths from Pneumonia		Deaths from Influenza		Deaths from Circulatory Diseases	
	1 1889-90	2 1861-80	3 1889-90	4 1879-88	5 1889-90	6 1879-88	7 1889-90	8 1879-88	9 1889-90	10 1879-88	11 1889-90	12 1879-88	13 1889-90	14 1879-88
	°	°												
49	41·7	41·7	188	207·8	28	26·4	31	35·3	7	8·4	0	0·3	9	11·1
50	41·8	41·5	218	217·0	18	28·6	39	40·5	10	8·2	1 ?	0·0	17	12·6
51	44·7	41·3	229	218·6	21	27·5	36	39·1	11	6·3	0	0·0	14	12·8
52	46·2	41·2	166	223·5	17	29·3	25	40·4	11	10·0	0	0·0	3	13·1
1	42·8	41·0	236	219·2	23	27·8	52	40·4	15	8·8	0	0·0	14	14·4
2	48·4	40·9	299	220·0	29	31·6	66	39·1	29	8·9	4	0·0	21	12·7
3	48·6	40·7	325	218·4	38	29·5	87	41·1	30	7·6	13	0·1	23	14·1
4	41·3	40·8	330	229·8	37	30·1	85	48·1	32	9·1	8	0·1	30	12·2
5	43·2	41·1	294	230·1	29	29·5	64	43·5	21	9·8	6	0·0	18	12·7
6	41·0	41·3	226	227·6	22	28·0	36	41·9	13	7·6	7	0·1	14	12·8
7	38·4	42·0	256	223·3	24	26·7	52	41·0	14	10·7	7	0·1	13	11·4

TABLE II.—*Showing the Influence of Temperature and of Influenza respectively on the Mortality from All Causes, and from Diseases of the Respiratory and Circulatory Organs in particular.*

PERIODS	Deaths from All Causes	Deaths from Respiratory Diseases	Deaths from Bronchitis	Deaths from Pneumonia	Deaths from Heart Disease	Mean Temperature
AVERAGE OF TEN YEARS, 1880–89.						°
1st week - -	219·2	55·3	40·4	8·8	14·4	41·0
2nd week - -	220·0	54·3	39·1	8·9	12·7	40·9
3rd week - -	218·4	56·4	41·1	7·6	14·1	40·7
4th week - -	229·8	65·5	48·1	9·1	12·2	40·8
5th week - -	230·1	62·0	43·5	9·8	12·7	41·1
6th week - -	227·6	58·7	41·9	7·6	12·8	41·3
Means -	224·2	58·7	42·4	8·6	13·2	° 41·0
COLD YEAR, 1881.						
1st week - -	203	47	35	5	14	41·3
2nd week - -	224	70	51	12	10	28·5
3rd week - -	292	91	74	6	20	27·3
4th week - -	303	117	93	12	11	32·9
5th week - -	297	111	85	13	18	42·2
6th week - -	267	91	77	7	11	41·5
Means -	264·3	87·8	69·2	9·2	14·0	° 35·6
WARM YEAR, 1884.						
1st week - -	193	41	29	6	17	44·2
2nd week - -	182	38	23	8	13	46·4
3rd week - -	180	39	31	5	13	46·9
4th week - -	165	43	32	3	6	45·1
5th week - -	200	39	28	5	13	41·8
6th week - -	202	37	24	4	14	46·0
Means -	187·0	39·5	27·8	5·2	12·7	° 45·1
INFLUENZA YEAR, 1890.						
1st week - -	236	75	52	15	14	42·8
2nd week - -	299	112	66	29	21	48·4
3rd week - -	325	130	87	30	23	48·6
4th week - -	330	130	85	32	30	41·3
5th week - -	294	89	64	21	18	43·2
6th week - -	226	56	36	13	14	41·0
Means -	285·0	98·7	65·0	23·3	20·0	° 44·2

This appears very clearly from Table II., in which I have given figures for the first six weeks of a very cold year, 1881; for the corresponding period in a very warm year, 1884; and for the corresponding period of an average of 10 years. The mean temperature of the first six weeks of 1881 was as much as 5·4° *below* the average (41·0°). The mean weekly number of deaths from all causes in that period were 40·1 *above* the average—264·3 compared with 224·2. The mean deaths from diseases of the respiratory organs were 29·1 *above* the average—87·8 compared with 58·7. The mean weekly deaths from bronchitis were 69·2, compared with a ten years' average of 42·4, while the mean weekly deaths from pneumonia were 9·2 compared with an average of 8·6. Who can hesitate to conclude from these figures that severe cold at the beginning of 1881 raised the death-rate from all causes, but more especially that from respiratory diseases, and among these notably bronchitis, and to a less extent pneumonia?

Now, take the converse. In the year 1884, the mean temperature of the first six weeks was 45·1°, or 4·1° *above* the average. The mean weekly number of deaths in that period were 37·2 *below* the average—187·0 compared with 224·2. The mean deaths from respiratory diseases were 19·2 *below* the average—39·5 against 58·7. The mean weekly deaths from bronchitis fell to 27·8 compared with an average of 42·4; those from pneumonia fell to 5·2 compared with 8·6. Is it not equally clear from these figures that mild weather in January, 1884, lowered the death-rate from all causes, and from diseases of the breathing organs among the rest?

And now we come to the opening six weeks of 1890, when the mean temperature shows an excess comparable with that of 1884—it was 44·2°, or 3·2° above the average, and only 0·9° below the value for 1884. Under these circumstances, a low rate of mortality from all causes, and especially from respiratory diseases, was to have been looked for. But how different were the facts! The mean weekly number of deaths were 60·8 *above* the average—285·0 against 224·2. The mean deaths from respiratory diseases were 40·0 *above* the average—98·7 against 58·7. The mean weekly deaths from bronchitis were 65·0, compared with the average, 42·4;

while the mean weekly deaths from pneumonia were 23·3, compared with an average of 8·6.

It is of interest to observe that, whereas the deaths referred to bronchitis were only 53 per cent. in excess of the average, those referred to pneumonia were no less than 171 per cent. in excess. Bearing this result in mind, we have a clue to the prime cause of the heightened death-rate in 1890, which was manifestly the epidemic of influenza. At a meeting of the Imperial Royal Society of Physicians of Vienna on January 31, 1890, Professor Weichselbaum* read a paper on Influenza, embodying the results of his investigations into the condition of the blood and catarrhal secretions of the respiratory mucous membranes of patients suffering from the disease, as well as the morbid products found *post-mortem*. In most of the cases the presence of the diplococcus-pneumoniæ was proved, microscopically as well as by cultures—at least, a capsulated diplococcus was discovered, differing very little from the micrococcus of pneumonia. From his investigations Professor Weichselbaum inclines to the opinion that the influenza was produced by other unknown organisms, the presence of the pneumococcus being due to a secondary infection. The view that only a favourable soil for the development of the pneumococci was created by the influenza is supported by the fact that these micro-organisms were also found in healthy persons. We now see why pneumonia is such a frequent complication, or sequela, of influenza; and further, how it was that pneumonia showed such a marked prevalence and fatality, as compared with bronchitis, in Dublin during the ever-memorable month of January, 1890.

During the eleven weeks included in Table I. only 45 deaths were registered as having been directly due to influenza. Of these, the first was registered so far back as the week ending Saturday, December 14, 1889. The victim was an infant under one month old, who lived and died in No. 4 South City (Grand Canal-street) Registration District. I cannot help regarding the influenzal theory of the cause of this infant's death as doubtful; it was not until the second week of the New Year that other deaths from

* Cf. Brit. Med. Journal, Feb. 8, 1890. Page 322.

influenza were registered in the Dublin District. In that week the deaths directly referred to the epidemic were 4 in number; the victims were—an infant under one year, two persons aged between 40 and 60, and one octogenarian. One of the deaths occurred in No. 1 North City District West, two in the Rathmines District, and one in the Donnybrook District.

In the third week of 1890, 13 deaths were attributed to influenza—2 patients being aged between 5 and 20 years, 3 between 20 and 40, 5 between 40 and 60, and 3 between 60 and 80. Four of the deaths took place in No. 1 North City District West (Langrishe-place); two in No. 3 North City District (Benburb-street); one each in Nos. 1 and 3 South City Districts, Clontarf and Howth, No. 1; Rathmines, Donnybrook, and Blackrock.

In the fourth week, the deaths were 8—3 being of persons aged 20 and under 40, 4 between 40 and 60 years, and one of a person aged 60 and under 80. The geographical distribution of the casualties was—3 in No. 1 North City District West (Langrishe-place), 1 in No. 3 North City District, 1 in No. 1 South City District, 2 in No. 4 South City District, and 1 in Kingstown.

In the fifth week, the deaths were 6—at ages 5 and under 20 (1 case), 20 and under 40 (1), 40 and under 60 (1), 60 and under 80 (2 cases), upwards of 80 (1 case). The 6 deaths were distributed thus—1 in No. 3 North City District, 2 in No. 4 South City District, 2 in Donnybrook, and 1 in Kingstown.

In the sixth week, the deaths were 7—1 between 20 and 40 years of age, 2 between 40 and 60, 3 between 60 and 80, and one upwards of 80. Two of the 7 deaths occurred in Langrishe-place District (No. 1 North City West), one in Lisburn-street (No. 2 North City), 2 in Grand Canal-street (No. 4 South City), and 1 each in Finglas and Glasnevin, and in Donnybrook.

In the seventh week, 7 deaths were registered as due to influenza—1 between 1 and 5 years of age, 1 between 20 and 40, 2 between 40 and 60, and 3 between 60 and 80. One death from influenza occurred in each of the following districts—No. 1 North City West, No. 2 North City, No. 1 South City, No. 3 South City, No. 4 South City, Donnybrook, and Kingstown.

I do not think that much importance is to be attached to the fact that, while 11 deaths were registered as due to influenza in No. 1 North City District West (Langrishe-place), only two such deaths were registered in the neighbouring district—No. 2 North City; or, similarly, that, while 7 deaths from influenza were registered in No. 4 South City District, only two such deaths were registered in No. 3 South City District. I compared the death-rate from diseases of the respiratory organs in these several districts during the first seven weeks of 1890, and find that it is about the same in all four districts—allowance being made for differences of population. It would really seem as if in some districts influenza, and in others bronchitis or pneumonia, had been given the more prominent place as the immediate cause of death. There can be no question, I think, that in many cases returned as deaths from bronchitis or pneumonia, influenza was the "primary cause"—to use the language of the medical certificate of the cause of death.

One interesting fact does, however, stand out in relief from the statistics given—namely, that influenza spread as early and as quickly through the suburbs as it did through the crowded streets and alleys of the city. This is in marked contrast to the behaviour of such infective diseases as typhus, small-pox, scarlatina, and measles, and may fairly be used as an argument in favour of the *pandemic*, as distinguished from the *epidemic*, spread of influenza.

Another point connected with the statistics of the epidemic, to which I would draw attention, is the influence it exercised over the deaths from affections of the circulatory system, and in particular of the heart.

In Tables I. and II. are included columns which contain the facts relative to the deaths from circulatory diseases, such as aneurysm, pericarditis, and "heart disease" in the most general sense. If we compare columns 13 and 14 of Table I., we are struck by the marked rise in the number of deaths coincident with the chief epidemic period—the 2nd, 3rd, and 4th weeks of 1890. In the fourth week, diseases of the circulation were returned as the cause of death in 30 instances, compared with a ten years' average of 12·2 deaths for the corresponding week. Of the 30 victims, 14 were aged between

40 and 60 years, 9 between 60 and 80, and 4 were above 80 years.

Turning to Table II. we see that even intense cold in winter does not materially affect the death-toll from diseases of the circulatory system—the mean weekly fatalities in the first six weeks of 1881—the cold season—being 14·0 compared with an average of 13·2. Again, warmth in winter does not seem to lessen the mortality from this class of maladies, for the weekly mean deaths were 12·7 compared with the average 13·2.

In the influenza year it is quite the reverse. The mean weekly number of deaths rises to 20·0 from an average of 13·2 in ten years.

One of the most interesting inquiries connected with the epidemic is the influence of age on the mortality from the malady itself, and from its complications and sequelæ. This is too large a question to discuss in the present paper. Suffice it to say that the influenza did not appreciably raise the death-rate among young children, while it was fatal to adolescents and adults in the prime of life, as well as after the age of fifty years. This will be clear from the following comparison of the deaths at certain periods of life in the last four weeks of 1889, and the first four weeks of 1890:—

TABLE III.—*Showing the Total Number of Deaths at Different Ages in the last Four Weeks of* 1889 *and the first Four Weeks of* 1890, *respectively.*

—	1889, Deaths	1890, Deaths	Percentage increase
Under 5 years - - -	242	236	—
From 5 to 20 years - -	61	88	44
From 20 to 40 years - -	127	236	86
From 40 to 60 years - -	168	304	81
From 60 to 80 years - -	165	278	68
80 years and upwards - -	38	48	26

From this Table it will be seen that the fatal January of 1890

slew fewer children under five years of age than the colder December of 1889 had slain.

From the foregoing considerations we may conclude that—

1. The epidemic of influenza was more pernicious to the population of Dublin than the extreme cold of January, 1881.
2. It slew its victims, not so much directly, as by means of complications and sequelæ, affecting the breathing organs and the heart.
3. It spared the lives of children of tender years, but killed large numbers of adults and those advanced in life.
4. Its effect upon the death-rate was sudden and pronounced, and lasted for at least seven weeks, or throughout the month of January and first half of February.

PART II.—CLINICAL.

It would be impossible, within the limits of this paper, to give the clinical histories of all the cases of influenza, amounting to about 100, which came under my immediate observation in hospital and private practice. Nor, indeed, have I materials at hand to do so, even did time and space permit. At one period—about the 10th of January—between forty and fifty new cases were daily prescribed for in the out-patient department of the Meath Hospital; and at the same time several cases of influenzal secondary pneumonia and bronchitis came under treatment at my hands in the hospital wards.

The earliest cases of which I had cognizance occurred on the 5th and 6th of December respectively. One of these two patients consulted me on Monday, December 9th, having been ill from Thursday, the 5th. He was a clergyman, aged fifty-two, in whose case the prominent symptoms were—headache, tender eyes, palpitation, insomnia, and loss of strength. The other patient was a young lady, aged nineteen, who was suddenly attacked with shivering, headache, and high temperature, with very rapid pulse, on Friday, December 6. The fever lasted a few days, and then gave way, to be followed by a second rigor on Friday, Dec. 20,

and urgent chest symptoms. On visiting her with Dr. Usher, of Dundrum, on the 22nd, her pulse was 124, respirations were 36, and the temperature 102·7° at 3 p.m. The left base was solid. Great dyspnœa, tubular breathing, and marked bronchophony were present. She was bringing up an abundant muco-purulent sputum—like that of bronchiolitis rather than acute lobar pneumonia. It was examined by Dr. Bewley, who reported that no tubercle-bacilli were present, but that various putrefactive microbes were found. On December 26 her pulse was 120, respirations had risen to 48, and the temperature at noon was 101·6°. Dr. James Little saw her with Dr. Usher and me next day, when we found a new patch of pneumonic consolidation in the middle of the right back, and there were a few rusty-tinged sputa. From this time she steadily recovered, and went to Bournemouth with her father on January 23rd, 1890, "pretty nearly all right," as he expressed it in a letter to me.

I propose now to give a brief clinical sketch of four cases—each illustrating a type observed in the epidemic.

CASE I.—*The Nervous, Neuralgic, or Rheumatoid Type.*—One of the earliest cases which I saw was that of a lady, who was seized on the evening of Friday, December 20, 1889, and who was seen next morning by my friend, Dr. James Craig, one of the Assistant Physicians to the Meath Hospital. The following is this lady's own account of her attack:—"Friday, Dec. 20th, 1889, I went to the oratorio at St. Patrick's Cathedral apparently in my usual health. Shortly after entering the Cathedral I felt chilled, as if cold water was being poured down my back and legs. When I returned home I warmed myself at a good fire, was given some hot wine and water, and went to bed; then my face and head got very hot and uncomfortable, and pains began in my arms, shoulders, and legs. All night the pains were very bad, sometimes so sharp across the back of my chest that I could have cried out; and, although I felt burning to touch, the cold-water sensation continued. I got no sleep that night. Next day, about twelve o'clock (mid-day), I was given a powder (salicylate of sodium) and in two hours afterwards another, which put me into a perspiration. The pains in my limbs got better, but my head began to ache badly and all day I felt very ill. I suffered from

great thirst. Saturday night slept better. Sunday morning about 5 a.m. I wished for a cup of tea, but could not taste it. I might have been drinking hot water. Sunday evening pains had quite gone. I had no headache. I got up for a while, but felt very weak. For several days I had no energy for anything—the least exertion tired me. My sense of taste did not return for four or five days. I also got a cough which was very troublesome. Temperature—Friday night 101°; Saturday morning 100°, evening 98·8°."

Case II.—*The Cardio-pulmonary Type.*—On Monday, December 30th, 1889, Mrs. W., a lady, aged fifty-four years, somewhat frail and delicate, while out walking was seized with shivering and violent headache, and intense pain in the back and in the "bones." On reaching home she at once went to bed, feeling very ill and prostrate. Next day I visited her. The tongue was thickly furred and dry. Her pulse was 132, respirations 28, temperature 103·3°. Having regard to the sudden onset of the illness and the symptoms, I pronounced the attack to be one of influenza.

On New Year's Day (third day) the pulse was 110, respiration 28, temperature 102·0°. The tongue thickly coated; eyes tender, and lacrymation; complete anorexia; great prostration.

January 2, 1890 (fourth day).—The report was that she had a better night. Herpes was showing round the nostrils. Pulse 96–100; respiration, 28; temperature, 102·4°. Severe stabbing or catching pain was complained of at the lower part of the left side of the chest. No physical signs could be detected, and a hot poultice relieved the pain.

January 3 (fifth day).—Pulse, 110; respiration, 32; temperature, 102·7°. A lymphy crepitation was now audible over the upper part of the left side of the chest, and on deep inspiration a fine pneumonic crepitation could be heard.

January 4 (sixth day).—Pulse, 110; respiration, 40; temperature, 103·2°. Dulness now existed, which was rapidly extending all over the left apex, where also a marked *frottement* could easily be recognised. At 6 p.m. Dr. Watson Pike, A.M.S., saw the patient with me and agreed in my diagnosis of influenza complicated with a left pleuro-pneumonia. Pulse, 112; respiration, 42; temperature, 102·7°. There was not a trace of expectoration, and scarcely any cough occurred. We considered the patient to be in danger, and continued the treatment, which consisted in free stimulation, frequent feeding, and quinine.

At 1 30 a.m. of Sunday, January 5, I was summoned to see the patient, and found her sinking fast. Dr. Hearn, of Rathmines, kindly joined me in consultation. Her pulse was failing, and the temperature was 103·3°. She rallied for a time, but at 6 a.m. another attack of cardiac failure came on. From this also she rallied, but at 10 a.m. she suddenly died.

As bearing on the diagnosis of this case, it is to be noticed that four—if not five—of the other members of this lady's family suffered from influenza either immediately before or after her illness.

Appended is the clinical chart. [See Plate I., Chart I.]

CASE III.—*The Gastric Type.*—On Wednesday, January 8, 1890, Mr. W. B. S. enjoyed a day's shooting in the Co. Wicklow. The following day he returned to town in his usual good health; but in the afternoon he felt chilly, complained of headache and nausea, and felt utterly miserable. He went to bed early, but passed a wretched night—restless and sleepless. Next morning I found him complaining of pains in the eyeballs, back of the head, and small of the back. Pulse, 84; temperature, 99·9°; tongue thickly coated; complete loss of appetite and nausea. He felt entirely prostrate, and, at my evening visit, expressed his belief that some fish which he had eaten for dinner had thoroughly disagreed with him. Two miserable days of sickness followed, the temperature rising on the morning of the 5th day to 103·1°. A short cough had set in, and the eyes were suffused and tender. There was constipation, and he complained of weight and fulness in the pit of the stomach. Dr. James Little saw him with me and thought it likely that the fever would run on for some time. A quiet day gave promise of a restful night, and this promise was abundantly fulfilled. He had an excellent night, partly due to 20 grains of antipyrin, with 20 minims of tincture of gelsemium in a draught in divided doses at bedtime. Next morning, pulse 76; temperature 98·7°, rising to 100·2° in the evening, but without any return of restlessness. Subnormal temperatures followed for a few days—96·4° being one observation. The tongue cleaned very slowly, and several days of extreme languor and weakness preceded final convalescence.

This gentleman's wife had, a few days previously, suffered from influenza, from which she was recovering when he fell ill. She

Plate I

EXAMPLES OF TEMPERATURE RANGES IN INFLUENZA OBSERVED IN DUBLIN.

CHART I.—Mrs. W., aged 54.

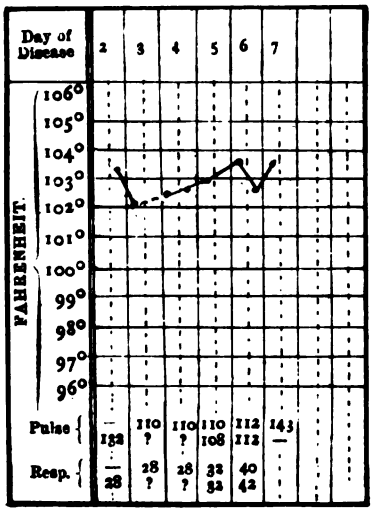


CHART II.—W. B. S.

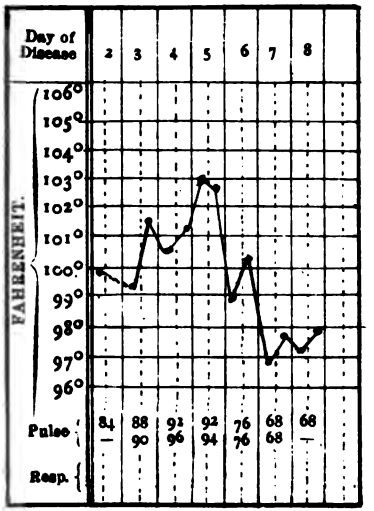


CHART III.—W. S.

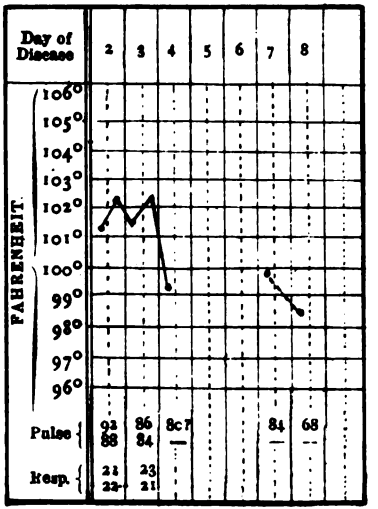


Plate II

EXAMPLES OF TEMPERATURE RANGES IN INFLUENZA OBSERVED IN DUBLIN.

CHART IV.—Elsie M.

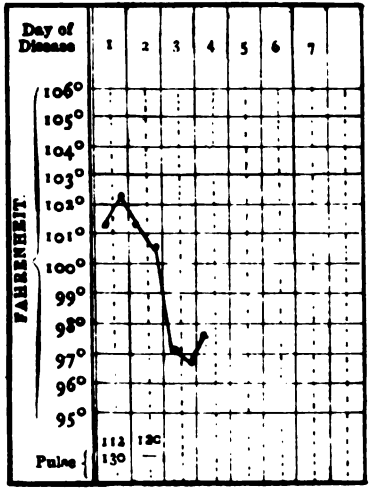


CHART V.—Dorothy O.

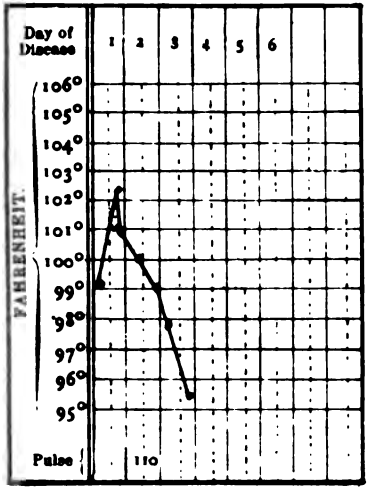
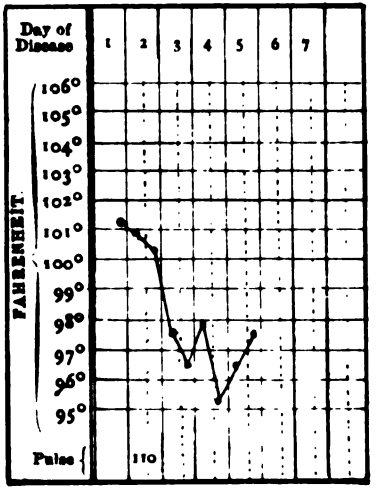


CHART VI.—Fanny C.



nursed him and got a relapse, accompanied with cough, bronchial catarrh and absolute loss of appetite. It is right to mention that Mr. S. had been subject to occasional "bilious attacks," as he described them. Except for these he had not known a day's sickness from his boyhood, and did not recollect when he had been obliged to keep his bed even for a day. [See Plate I., Chart II.]

Case IV.—*The Febrile Type.*—On New Year's Eve I received a note from a surgical colleague asking me to see him as he had been taken ill the same afternoon while in his study. On visiting him in bed, he was still shivering at times, and complaining of a distressing feeling of cold water streaming down his back. He stated that he had been out of sorts for three or four weeks, and it was quite evident that he had made up his mind that the attack was one of typhoid fever. His pulse was 92, the temperature about 100°. His tongue was very furred, and his eyes were injected with swollen lids. He had lost the senses of taste and smell, and complained much of rheumatoid or neuralgic pains in the back and limbs. Two restless feverish nights followed. So I asked our mutual friend, Dr. Gordon, to see him with me. At the end of the examination, the patient said to Dr. Gordon—"Our friend Moore says I have influenza; do you think I have?" The answer was—"I do not think it, but I am sure of it." [See Plate I., Chart III.]

There was in this case steady pyrexia for four days, and then came profuse sweatings, lasting for several days. A slight elevation of temperature occurred on the evening of the seventh day, after which convalescence went on uninterruptedly. The weakness was for a time extreme.

This febrile type prevailed especially among children. The three charts, included in Plate II. and numbered IV., V., VI., show the febrile movement in girls, aged from 12 to 15 years. The marked subnormal temperatures on the third and following days are very noteworthy, and are so constantly present in the defervescing stage of influenza as to become an important diagnostic sign.

The lessons which personally I have learned from the epidemic may be stated in the form of propositions, as follows:—

1. Influenza is an acute specific infective disease of the mias-

matic, rather than the miasmatic-contagious class. Its virus or contagium, when once introduced into the body, acts primarily and quickly on the nervous system, producing the phenomena of an acute pyrexia, with singularly rapid pulse. Unlike the poison of typhus, the virus of influenza is not rendered inert by oxidation from contact with atmospherical air, but rather the contrary.

2. The disease appears to be *pandemic*, rather than *epidemic*, affecting multitudes at one and the same moment, both by sea and land—a known fact, which suggested to Dr. Hilton Fagge (who, however, does not adopt the miasmatic theory of the origin of influenza) the view that the organisms which give rise to influenza, if organisms there be, cannot undergo multiplication and development anywhere except in the air itself. The virus of influenza is then a miasma, or what the physicians of the sixteenth and seventeenth centuries called a "fouling of the air."[a] In this connection, Hirsch, of Berlin, points out that influenza has not spread more quickly in our own times, with their multiplied and perfected ways and means of communication, than in former decades or centuries.[b] The prevalence of the disease is absolutely independent of season and weather—a fact which distinguishes influenza from epidemic bronchial catarrh.[c] "Et tempore frigidiori et calidiori, et flante tam Austro quam Borea, et pluvioso et sereno cœlo, peragravit hasce omnes Europæ regiones, et omnia loca indiscriminatim."[d]

3. If this miasmatic or pandemic view of the origin of influenza is correct, there is no need to seek for a period of incubation, the virus being already "hatched" at the time of its reception into the human system—that is, at the time of infection, or, rather, of intoxication. In several, if not in most, cases, there is an interval between the reception of the poison and the development of the

[a] Hirsch. Handbook of Geographical and Historical Pathology. Vol. I., p. 34. New Syd. Soc. 1883.

[b] Loc. cit. P. 36.

[c] Cf. Hirsch. Loc. cit. P. 26.

[d] Petrus Salius Diversus, cited by Dunning (Med. and Phys. Journal. Vol. X., p. 43), and quoted by Dr. Thomas Hancock in an excellent article on Influenza in the second volume of the Cyclopædia of Practical Medicine, published in 1833.

symptoms. The most common duration of this interval seems to be one or two days. But this pseudo-incubation period may be explained on the supposition that in certain individuals an intact condition of the mucous membranes may present an obstacle to the entrance of the virus into the blood, and so delay the development of the disease. It is reasonable to suppose also that—whether we admit the doctrine of *phagocytosis* to be true or not—the resisting power of some individuals may postpone an attack for even two or more days.

Of course, it is not denied that the morbific agent or virus is capable of adhering to the human body, or to clothes, or luggage, or letters, so as to be conveyed from one place to another (Hilton Fagge). "But," adds that writer, "its subsequent growth and development is, doubtless, altogether independent of this kind of assistance."

I have observed several cases of apparent communication of the disease from person to person, but without being able to calculate the duration of a supposed period of incubation. In one such case, a lady visited a friend ill of influenza at 2 p.m., and was, three hours later, attacked with symptoms of the disease—chills, weakness, coryza, lacrymation, stuffing of the nostrils, and loss of smell and of taste. Here, doubtless, the virus clung to the person of the first patient, and was received fully developed into the system of the second, producing its toxic effects almost at once.

Several writers advance the view that influenza is both a pandemic—or miasmatic, and an epidemic—or miasmatic-contagious, disease. Thus, Dr. P. Duflocq, Chef de Clinique in the Faculty of Medicine in Paris, concludes an elaborate article[a] on the clinical varieties of influenza observed in that city in December, 1889, and January, 1890, with these words:—

"La grippe semble donc être une maladie à la fois épidémique et contagieuse, et la période d'incubation serait de deux jours."

4. Very young children seem to enjoy a certain immunity from influenza, or to have the disease in a mild form—that of an ephe-

[a] Des Variétés cliniques de la Grippe à Paris en Décembre 1889 et Janvier 1890. Revue de Médecine. Tome X. Page 85. Février, 1890.

meral fever, followed by profuse sweating, and, after a few days, a tendency to slight catarrh. In 1847 Dr. Fleetwood Churchill communicated to the *Dublin Journal of Medical Science*[a] observations he had made on the epidemic influenza observed in January and February of that year among children in Dublin. In upwards of 60 cases, embracing children of all ages, from two months old to twelve or fourteen years, he had not a single death; so that he was led to conclude that, among children, although the epidemic was very general and severe, yet the danger was not very great, when the disease was properly handled.

In January, 1890, all my five children had influenza, their ages ranging from 13 to 4 years. The mildest cases were those of the eldest and youngest. A boy of seven and a-half years had incessant vomiting and nausea for 24 hours, and profuse sweating, with a pulse as fast as in scarlet fever—140 per minute. There was a bronchial catarrh in two of the cases. This "child-type" of the disease presented itself to my notice also in a large girls' school, of which I am one of the physicians.

5. Adults suffer severely in many cases, the symptoms being—chills, headache, often sleeplessness, sometimes delirium, pains in the eyeballs, nape of the neck, small of the back, knees, and along the margins of the ribs, loss of the special senses of smell, taste, and, sometimes, hearing; smarting of the eyes, photophobia, lacrymation, otalgia, complete loss of appetite, bad taste in the mouth, nausea, and, perhaps, vomiting; constipation, but, occasionally, diarrhœa; cough, frequent sweating, loss of strength, fainting. Of course, it is only a selection from these symptoms that is present in a given case.

6. Influenza, while infrequently directly fatal, causes an indirect loss of life which is appalling, chiefly through complications affecting the respiratory, and, in advanced life, the circulatory systems. It has been said that influenza, while relatively less fatal, is absolutely more fatal than cholera.[b]

[a] Vol. III. May, 1847. Page 373.

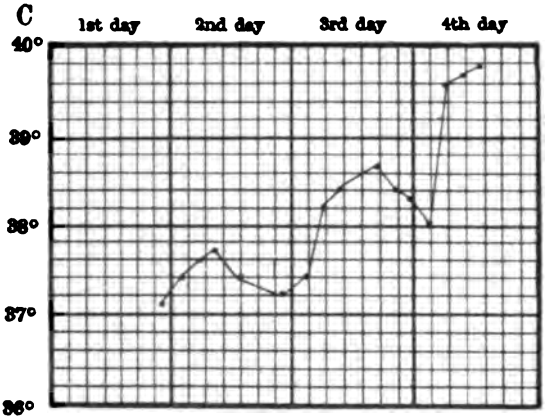
[b] Cf. Sir Thomas Watson. Practice of Physic. Third Edition. Vol. II., page 46. 1848.

Plate III

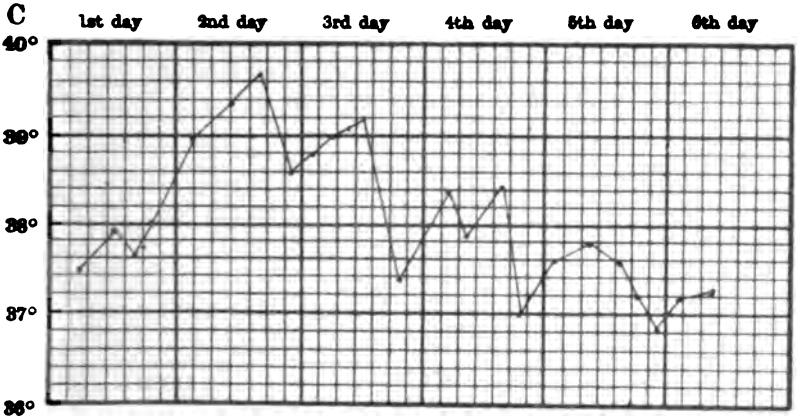
TYPES OF TEMPERATURE RANGES IN INFLUENZA.

(After Dr. Otto Frentzel, of Friedrichshain.)

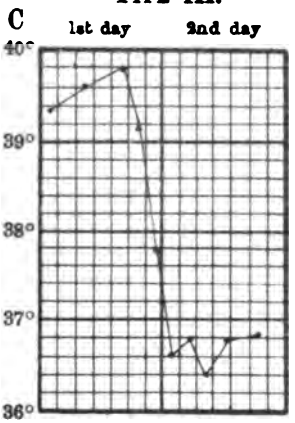
Type I.



Type II.



Type III.



I have seen fatal cases of influenzal bronchitis, pneumonia, pleuritis, and heart failure. The pneumonia, while producing the ordinary physical signs of acute croupous pneumonia, is often latent in its course, or accompanied by a profuse muco-purulent expectoration, with scarcely any rusty sputa. The ebbing of the strength in some of these cases in elderly people is something awful—it is often absolutely beyond control.

7. Influenza is a perilous complication of pulmonary consumption.

8. Other complications of which I have had experience are—Epistaxis (one case), facial neuralgia (several cases), profuse sweatings (several cases), skin-rashes (four cases—three were examples of papular sweat rashes, with sudamina; one was an erythema fugax), herpetic eruptions (several cases); cystitis, followed by mild orchitis (one case).

In contrast to Dengue Fever, I believe that influenza is a non-eruptive fever. When rashes do appear, they are accidental rather than essential, or specific, and they result from hyperpyrexia, or profuse sweating, or from the ingestion of such drugs as quinine, or antipyrin, or salicylate of sodium.

9. Influenza seems to have the property of picking out the weak point in an individual's constitution. If the patient is neurotic, nervous and neuralgic symptoms are likely. Any old tendency to catarrh of either the respiratory or the digestive mucous membranes is at once intensified in the presence or in the wake of this strange malady.

10. The febrile movement in even uncomplicated influenza is, as Wunderlich would say, "polytypical," or "atypical." This is shown by the charts already given, and, further, in a diagram containing three different types of temperature range in influenza (which I have had reproduced in Plate III.), illustrative of a paper by Dr. Otto Frentzel, Assistant Physician to the Municipal General Hospital at Friedrichshain, which appeared in the *Centralblatt für klinische Medicin*, for January 11, 1890.[a]

11. Influenza shows a marked tendency to relapse, and to this

[a] Zur Kenntnis des Fieberganges bei Influenza.

is largely due the indirect fatality of the malady. In the epidemic of 1847, the death-rate was estimated at 2 per cent. of the cases observed in London. It has certainly not been higher in the present epidemic. A fortnight, at least, elapsed last December between the occurrence of the earliest cases and fatalities from influenzal pneumonia and bronchitis.

12. The treatment of the affection turns upon common sense principles. It is expectant, palliative, and symptomatic. There is no specific for influenza; but the most useful drugs to employ in its treatment are—(1) quinine, (2) antipyrin (except in young children and the weakly), (3) salicylate of sodium, especially in effervescence, (4) phenacetin, and (5) effervescing citrate of caffein.

A FEVER RESEMBLING DENGUE, OBSERVED AT KELLS, CO. MEATH.

By JOHN RINGWOOD, L.K.Q.C.P., L.R.C.S.;
Medical Officer, Kells Union and Fever Hospital.

[Read in the Section of Medicine, February 28, 1890.]

DURING the summer of 1885 I met with several unusual cases of fever, which, after careful observation, I diagnosticated as dengue or break-bone fever. These scattered cases were evidently a milder modified form or sequela of a virulent bilious relapsing fever which had been epidemic in Kells for the previous six months.

Some of my cases during the outbreak of bilious fever were so severe that they were in no respect different from genuine yellow fever—resembling the cases of yellow fever so fully described by Dr. Stokes and Dr. Graves as having occurred in Dublin during an outbreak of bilious fever in 1826.

At the meeting of the British Medical Association held in Dublin in 1887, I read a paper on the cases of bilious relapsing and yellow fever which came under my notice during that period. I was to have read a paper at the next meeting of the British Medical Association held in 1888 on the further history of this outbreak, but was prevented from doing so by a serious illness; in that paper I would have given the history of the succeeding outbreak of dengue, which was the first ever observed in the British Isles. And now I shall endeavour to lay before you (the Fellows of the Royal Academy of Medicine in Ireland) the consecutive history of this dengue fever, which has been under my constant observation for the last four and a half-years, during which time I have treated over five hundred well-marked cases; and having seen in the last four months numerous cases of so-called influenza I must confess that up to this I have seen no new symptoms which

would lead me to suppose that the disease is a different one from that which I have carefully studied and reported on during the past five years. Of course, as I shall now show, the disease has not been identical in every case; it has varied with each season, as every epidemic does, but still underlying all its varieties the dengue character has been strongly marked.

Cholera and yellow fever, whose permanent habitations are in the torrid zones, sometimes visit these shores, and why should we consider ourselves exempt from the visitation of their milder predecessor, or successor, dengue fever?

The disease was introduced into the neighbourhood of Kells early in 1885, on the return of our troops from Egypt, from which time till midsummer I treated 220 cases, including 6 of yellow fever. Its type on its first introduction was that of bilious relapsing fever until the summer of 1885, when the relapsing fever became gradually milder, the type of the disease lost its malignancy, and the bilious vomiting and jaundice became secondary symptoms. Several cases came then under my notice of patients who after having been exposed to the contagion of the relapsing fever, developed the following symptoms after an incubation period of about four days:—Sudden onset, giddiness, syncope, intense frontal or occipital headache with great prostration, pain in eyes, neck, back, bones, muscles and joints; hands and feet stiff and painful, skin hot and dry, tongue yellow and coated; severe rigors, gastralgia, tympanites, rigidity of abdominal muscles, colicky pains, constipation, motions fetid, epistaxis or other hæmorrhages; face flushed, swollen and puffy, with a bright scarlet triangular patch on each cheek, the apex of each triangle meeting across the bridge of the nose; joints slightly swollen, temperature high, pulse 100; after three days' sharp sweating crisis, disappearance of febrile symptoms, apparent convalescence, pulse soft and slow and temperature subnormal; relapse on fifth day with an eruption, accompanied by enlarged glands, followed by free desquamation and tedious convalescence.

I now beg to lay before you the following case:—

Case.—A. B., aged twenty-four, healthy male, was on the 11th of July, 1885, suddenly attacked with such giddiness and vertigo that he staggered as if intoxicated; this was soon followed by stiffness in hands and feet, intense frontal headache, with pain at back of eyeballs, photophobia, slight nausea, skin hot and dry, tongue covered with yellow slime, papillæ enlarged. 12th—Bad night; could not rest with intense pain in back, which felt as if it was being sawn across; complained of feeling sick and giddy; pulse, which had been full and strong the evening before, now soft and weak; hands stiff, swollen and painful; bowels confined, abdominal muscles rigid, epigastrium tender. 13th—Better night, but utterly unable to move from intense occipital pain and crick in neck; eyes sore and smarting, cheeks puffy, and red patch extending across bridge of nose. 14th—Crisis, with profuse perspiration, rapid fall of temperature to subnormal, feverish symptoms gone, tongue clean, pains gone. 15th—Feels well after a good night, with difficulty kept in bed; pulse slow, temperature subnormal, feels only giddy on sitting up, appetite returning. 16th—Had rigor at 1 o'clock a.m., with rapid rise of temperature; return of feverish symptoms, eyes smarting, conjunctiva yellow, bowels confined, complains of nausea and pain over bladder, eruption rapidly extending from forehead over face and neck, glands in neck tender and swollen, tongue tremulous, muscles of hands in constant state of tremulous spasms, intense neuralgic pain shooting through spine, brachial and sciatic nerves; throat slightly sore, and difficulty in swallowing from spasm of the muscles of deglutition. 17th—Whole body covered with eruption resembling that of scarlatina; could not rest with constant itching and tingling of whole surface of body; greatly annoyed by flatulence and upward movement of the diaphragm; feeling of faintness and utter prostration, with intense thirst. On 18th had favourable change, fever subsiding, desquamation commencing. On 20th, slow convalescence, no further relapse or complications. The temperature chart of the above case gives a good idea of the disease in its simplest form.

In this case the eruption resembled that of scarlatina, but in other cases it resembled that of measles, urticaria, prickly heat, or chicken-pox, and in cases with several relapses the eruptions in each relapse assumed a different character; where there was intense pain and swelling of joints without any eruption the disease re-

sembled rheumatic fever with relapses; when the joint pains were similar and the eruption like scarlatina the disease closely resembled rheumatic scarlatina, under which name dengue has been described; when eruption was limited to mouth and throat the disease closely resembled rheumatic tonsillitis; when eruption extended to throat and Eustachian tubes and frontal sinuses it gave rise to sore throat, abscess in internal ear, ear vertigo and intense frontal headache; when further spread it usually, at least in primary fever, extended to the larynx and down trachea and bronchial tubes, giving rise to laryngeal irritation, spasm, incessant cough and aphonia, tracheitis and capillary bronchitis, with shifting patches of pulmonary congestion. If, on the other hand, it followed the alimentary track, it caused œsophagitis, with painful spasmodic difficulty in swallowing, acute gastralgia, with intense burning pain in stomach, duodenitis, with bilious vomiting and slight jaundice, enteritis, with constipation, flatulence, and intense burning pain in rectum, with tenesmus and occasional hæmorrhage. When the mucous membrane of the genito-urinary track was affected, which sometimes occurred apparently from metastasis of the skin eruption, kidneys were congested and tender, with pain in loins, cystitis and urethritis in both sexes; inflammation of Cowper's glands and orchitis in males; vaginitis, menorrhagia, pyosalpinx, and ovarian inflammation in females, showing the close analogy of these organs in the different sexes. When the full force of the disease fell on the nervous system, intense frontal headache, agonizing backache, neuralgic pains in muscles, tenderness of superficial nerves, cramps in calves of legs, tingling, burning and stiffness of hands and feet, and sometimes of the whole body, resulted; this stiffness often followed by transient quivering of muscles of extremities, drop-wrist and weakness of extensor tendons of feet. The giddiness, ear vertigo, mental confusion, at onset were purely nervous affections, as were also the rigidity of the abdominal muscles, the peculiar twisting movements and painless muscular spasms of the intestines. The jaundice was certainly often caused by nervous spasm of bile duct. The irregular pulse, varying not so much in rhythm as in fulness and force, with the cardiac oppression, angina, and breath-

hunger, were certainly the results of the effects of the poison on the sympathetic nervous system. The herpes on lips, ears and nose, the herpes zoster over course of painful nerves, the brachial and intercostal, were similar results of the nervous implications, as were also the constant tendency to syncope, utter prostration, deathlike coldness of knees and surface of body, total absence of delirium and subnormal temperature.

Dengue, as observed by me in 1885, was, in uncomplicated cases, a short fever of about eight days' duration, divided into three nearly equal stages—first, with intense general pains, high fever, pathognomonic patch on cheeks and nose and peculiar form of conjunctivitis; second stage ushered in by sweating crisis, with absence of fever, and generally subnormal temperature; third stage, relapse, with skin eruption, enlarged glands, high fever and desquamation.

In 40 per cent. of my cases eruption appeared on skin during third stage, and slight jaundice in 16 per cent.

The principal complications which I observed at this time were—relapses, septicæmia, phlebitis, giving rise to phlegmasia dolens; and in one case of a feeble old man gangrene of the right foot. Abscess in ears frequently occurred, also in upper or lower eyelids of left eye in four cases. Strange to say, I never saw any of these abscesses in right eyelids. Embolisms in lung tissue gave rise to abscess in ten cases, four of which terminated in phthisis. Large anthraces often greatly delayed convalescence; and in numerous cases herpes on lips extended into the mouth, giving rise to aphthous ulcers on inside of cheeks and tongue, and in two cases to cancrum oris and glossitis. These aphthous ulcers often appeared about the nose and anus.

About this time I observed that pregnant women did not abort when attacked by dengue—a result which always occurred in those attacked by the preceding bilious relapsing fever. During the winter of 1885 the number of cases greatly lessened, and the disease became freer from complications, but the pains in the muscles and bones so severe as to well merit the American name of break-bone fever. Where the sanitary arrangements were very defective an

occasional case occurred of the original relapsing fever, showing the close connection between the two diseases.

In the spring of 1886 diphtheria became a frequent complication of third stage of the disease; and during the summer of that year, when there was a great lull in the activity of the disease, I met with several cases of a low protracted illness, with slight jaundice, pain under left lower ribs, with splenic enlargement; ulcers on legs, and general debility, with slight feverish exacerbations and remissions, perhaps caused by minute hepatic abscesses. These cases occurred amongst those who had escaped attacks of either fever.

In the winter of that year the pulmonary complications became more pronounced, and a contagious form of broncho-pneumonia with intermissions and relapses often occurred amongst those exposed to the dengue contagion; and in these cases I found that the cough, dyspnœa, &c., bore not the slightest proportion to the amount of lung actually implicated. Pulse in these cases varied greatly—now full, strong and bounding, and then, in a few hours, weak, empty and irregular; respiration slow or rapid; expectoration sometimes nil or only mucous, free from air bubbles; inflamed portion of lung sometimes slowly degenerating into abscess, or becoming the starting-point for tubercular mischief.

During the summer of 1887 the bilious element again predominated, and diarrhœa for the first time took the place of constipation—rather an advantage, as few cases in which there occurred spontaneous diarrhœa had any relapse.

The disease seemed to smoulder until the end of 1887, when it burst out again in full activity—the first period of the disease having nervous symptoms most strongly marked, and the secondary eruption having all the characteristics of malignant measles, complicated with empyema, abscesses in lungs, septicæmia, multiple abscesses in joints, and tendency to relapse after relapse, with a different form of eruption in each relapse.

The following case I have already published:—

A little girl, aged fifteen, had first an attack of dengue, with measly eruption, followed by relapse, with eruption resembling

scarlatina, which greatly affected throat, and was complicated by a sharp attack of diphtheria and albuminuria; while freely desquamating a third relapse occurred, with a pustular eruption, the pustules actually pushing off flakes of the desquamating skin.

The area over which this ebulition of the disease spread was greatly extended, and I had then under my care 110 cases of the disease. The affection gradually died out again, and I saw a case only now and then, just enough to keep it alive, though this was hardly necessary, as, by this time, every acute disease had taken on a dengue type and become intermittent—a very unusual phase in this climate.

Towards the winter of 1888 a contagious form of pleuro-pneumonia of the right lung, with bilious complications, came under my care, in several cases ending in effusion, serous or purulent. At this time I had six cases of pericarditis, and five of peritonitis, showing that for the first time the poison of the disease had fallen on the serous membranes.

In the spring of 1889 I had several well-marked cases of dengue, with severe lung complications and relapses, one of which Dr. Magee Finny saw with me in consultation.

During the last four months a large percentage of the population here has been affected with the disease in various forms—few, indeed, escaping without at least a slight attack; and several who had gone through one or two attacks during the last five years being now similarly affected, proving that former recent attacks in no way confer protection on individuals or the community at large. The present outbreak has the characteristics of those of the three last winters combined.

I have seen in the last four months, in the Kells Fever Hospital or in private practice, an average of 50 patients a day severely enough affected to be confined to their beds, and amongst all these I have not met with a single case exhibiting symptoms or complications which have not come under my notice in the last five years, and been described by me in this paper. So that the conviction forces itself upon me that this neighbourhood has never been free for the last five years from either influenza complicated by dengue,

or dengue with modified cases, which are commonly called influenza. That the latter conclusion is the correct one the history of outbreaks of dengue in tropical regions and America fully corroborates.

Dr. Christie, Lecturer on Public Health, Anderson's College, Glasgow, seeing a report of mine in one of the recent Medical Journals that dengue had appeared in these northern latitudes, kindly sent me his paper on an outbreak of dengue which had come under his observation in 1870 in Zanzibar. In this paper he gives all the symptoms of the disease as exactly described by me as occurring in the typical cases of dengue which came under my observation in 1885. He states that the triangular red patch on each cheek, with apex across bridge of nose, is pathognomonic of dengue.

Limited time prevents me from alluding further to his most interesting paper.

With regard to this red patch on the face, allow me to state that in the cases of yellow fever which I treated here in 1885, I reported that one of the most characteristic features was the purple patch (nearly black) on cheeks, extending across nose, together with the purple ears. In my cases of bilious relapsing fever the tint of these patches was dusky red with ears still purple; while in my cases of well-marked dengue these patches were brilliant scarlet, and the ears were congested and often covered with herpes. In each of the three diseases we had sudden onset, high fever, rigidity of abdominal muscles, colicky pains, tympanites, constipated bowels, and jaundice, varying only in degree. Sudden death from syncope was always a present danger in all three diseases; utter prostration of mind and body and total absence of delirium, sweating crisis, feverish intermissions, with subnormal temperature and relapse, followed by tedious convalescence and the same complications, make the diseases one and the same, varying only in degree as to severity and the part of the system affected; in yellow fever the ganglionic nervous system—in relapsing fever the glandular system—and in dengue fever the nervous and cutaneous system—bearing the brunt of the fever.

In an account of an outbreak of dengue fever which occurred between November, 1888, and September, 1889, on board H.M.S. Agamemnon, stationed at Zanzibar, reported by Dr. Godding, and published by the *British Medical Journal* of the 15th of this month, there were 175 cases of bilious fever during the first six months before a typical case of dengue occurred—the same time as elapsed before my cases of bilious fever took on the pure dengue type.

Uncomplicated dengue in healthy subjects and with favourable surroundings is never a fatal disease, still, even in mild cases, it leaves behind it a vitiated state of system and great debility; whilst occurring amongst the old or delicate it is a most dangerous disease—with old men markedly so, old women generally recovering. During the last month I have treated four old women whose ages ranged from 89 to 94, who have successfully battled against severe typical attacks.

The disease is certainly contagious, the germs clinging for a long time to walls, bedding, &c., &c., and being unaffected by ordinary disinfectants. Horses, cats, and dogs readily take the disease from human beings, and when affected spread the disease amongst their own species as well as human beings. I have already reported such cases, but the limits of this paper prevent my doing more than alluding to the subject.

Dengue in tropical climates usually follows in the wake of either cholera or yellow fever epidemics. A severe outbreak of cholera occurred in 1821, followed by dengue in 1823. The cholera of 1870 was followed by dengue in 1871. The yellow fever outbreaks of 1825, 1843, 1850, 1880, were each followed by epidemics of dengue. The outbreaks of bilious and yellow fevers in Dublin in 1826, described so graphically by Dr. Graves, was followed by cases with rheumatic pains, jaundice, and eruptions, which certainly bear a great resemblance to the dengue which was then very prevalent in America and Africa. The cholera in Dublin in 1832 was followed by influenza in 1833, when dengue was prevalent in Australia and Africa. Yellow fever in Scotland in 1843 was followed by influenza in 1844, at which date dengue was epidemic in America. The relapsing and yellow fevers in Dublin in 1846 was

followed by influenza. Relapsing and yellow fevers in Kells in 1885 was followed by dengue, which has been endemic ever since. During same period dengue has been prevalent in Australia, Africa, America, and, as far as I can gather, is now everywhere epidemic.

I think the above dates clearly prove that, when dengue, the successor of cholera or yellow fever, is widespread over tropical countries far beyond the limits of its originating and more virulent predecessors, the British Isles are visited by so-called influenza, which I maintain is dengue, modified by climate, season, sanitary surroundings, and the individual susceptibility and suitability of the system either for the reception, retention, multiplication, or rapid elimination of the fever microbes. If these microbes are rapidly eliminated from the system through the pores of the skin or the mucous membrane, by either profuse perspiration, spontaneous diarrhœa, or mucous discharge from respiratory tract, the disease may terminate as an ordinary attack of influenza; a similar result may be, of course, obtained if the proper pabulum of these microbes is exhausted during the first period of the disease. In either case we have a short febrile disease of about three days' duration with all the symptoms of so-called influenza.

During the afebrile period that follows each crisis a fresh batch of germs is undergoing a process of incubation, which on coming to maturity in about thirty-six hours, give rise to another relapse and period of sharp fever—that is, if they still find a supply of suitable pabulum in the patient's system—this period lasting till a crisis again occurs; and these relapses may occur over and over again till the patient's system at last affords no more nutriment for the microbes.

During the first period of the disease, I believe the microbes are being constantly expelled through the pores of the skin and mucous membranes, giving rise to no apparent local irritation except on the face and hands, where the skin has become hardened and the pores are obstructed by constant exposure. The first sweating crisis carries off myriads of microbes, which pass freely through the open ducts, but in the relapses it is far different; the ducts are now clogged, and the attempts of nature to eliminate the microbes

through skin and mucous membrane give rise to various eruptions, followed by free desquamation; and in each succeeding relapse a different eruption results, as if the expulsion of the microbes incapacitated for some time a certain portion of the skin from acting as an eliminating medium. This theory accounts for the different eruptions on the skin and mucous membranes; also for the rapidly shifting patches of pulmonary congestion that I have observed in several cases, and which I have long looked on as erythema of the pulmonary mucous membrane, caused by the irritating presence of these microbes. The overwhelming action of these germs upon the nervous system, and the sad havoc wrought by them on that system, must never be overlooked, and call loudly for sustaining treatment of every description. But the limits of this paper preclude my touching, even in the briefest manner, on the treatment which I have found to be most suitable in the different phases of this disease.

MASSAGE.

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[Read in the Section of Medicine, April 11, 1890.]

The first case in which I employed massage was in the year 1883, and as I have had recourse to it somewhat extensively ever since, I thought it might be of interest to the Fellows of the Royal Academy of Medicine if, in the course of a communication before this Section, I were to give them the results of my experience. I am aware that when a member of our profession warmly advocates any method of treatment, especially if it be considered a novel one, he runs the risk of being dubbed a "faddist," and exposes himself to the charge that he treats everything by the one method, and that he has run away with an idea. Some of you will remember at the Surgical Society how the advocates of antisepsis in surgery were once described as sheep following a bell-wether. Similar charges may be brought against the advocates of massage, though perhaps in different language, especially if the advocates display the least enthusiasm. Well, I believe that members of our profession have a right—nay, ought to be enthusiastic over any method of treatment which promises to be remedial, provided that they satisfy themselves first that the method has a solid and scientific foundation—that they are sure of the ground upon which they stand.

In the first place, I hope to show you that massage is not a novel method of treatment—that, in fact, it is nearly as old as the history of medicine itself, that it has been known and practised in all countries and at all times, that it fell into disuse as a part of legitimate treatment, owing chiefly to the fact that it was allowed to slip gradually into the hands of charlatans and quacks, but that

in recent times it has been placed on a scientific basis, the indications for its use are being studied, that it is capable of very large application both in medical and surgical practice—sometimes with the most astonishing results—and that it behoves us to keep it above suspicion, within the realms of legitimate and scientific medicine, and not to allow it to degenerate into a system of empiric manipulations, the hope of the bone-setter and the refuge of the quack.

The word "massage," immediately derived from the French word "masser," to rub, to knead, owes its origin, I believe, to an Arabic root; it implies a rubbing, kneading, stroking of the body—in short, all those manipulations which have come to be understood by the word *massage*. The employment of the process itself, both as a hygienic and therapeutic measure, dates back to the earliest times. In the Odyssey we read that the women rubbed and kneaded the bodies of the heroes, anointing them with fragrant oils; and Hippocrates, 380 B.C., wrote that "the physician must be experienced in many things, but assuredly also in rubbing, for rubbing can bind a joint that is loose, and loosen a joint that is too rigid. And again, rubbing can bind and loosen, can make flesh and cause parts to waste. Hard rubbing binds; soft rubbing loosens; much rubbing causes the parts to waste; moderate rubbing makes them grow." And again he writes, although he must have been ignorant of the circulation of the blood and the direction of the lymph channels—"that rubbing upwards in the case of the limbs has a more favourable effect than rubbing downwards." Thus we see that hygienically and therapeutically the art of massage was well understood and valued by the antients. It formed an essential part of the celebrated baths so generally employed among the Greeks and the Romans. Under Nero, Domitian, and Trajan, the bathing establishments were constructed in the most convenient and luxurious manner. They consisted of six large apartments in which the various manipulations belonging to the baths were carried out. First, in the 5th apartment, which was called "Tepidarium," the bathers were made to perspire freely by means of certain gymnastic exercises. They were then stroked and rubbed down over

the whole body with peculiar skin rubbers (Strigiles) by slaves. The Masseurs, or Aliptæ, as they were called, then came upon the scene, and they pinched and kneaded the body, ending up by bending all the joints and stretching them until they cracked (*Ac summum dominæ femur exclamare coëgit*, as it is called in Juvenal, Sat. VI., 423). After this the Alipili appeared, and with small nippers removed the hairs from the body, after which the Unctores anointed the body with fragrant oils.

The process, which we call massage, was not, however, confined to the Greeks and Romans. It was employed as a means of cure by the wise men of India at the period when Alexander the Great entered their country. Lepage, in 1813, mentions, in his historical account of medicine among the Chinese, how massage was employed among them and had been borrowed by them from the Hindoos in antient times. The Chinese rub the whole body with the hands, softly squeeze the separate muscles, and pull at all the joints. Again, in 1845, Dr. Wise published at Calcutta a commentary on the Hindoo system of medicine, in which the duty is urged to rise early in the morning, to cleanse the mouth, to anoint the body, then to exercise and to undergo shampooing. Again, the historian Forster, who accompanied Cook on his travels, gives an account of their reception by a friendly chief in the island of Tahiti:—"In one corner of the hut a mat is spread out on the dry grass. A great number of our friend's relatives came, and the chief's daughter, who excelled in beauty by the loveliness of her features, the voluptuousness of her form, and the whiteness of her complexion, smiled at us in a friendly way, and made many efforts to prove herself agreeable. In order to remove our weariness she rubbed our arms and legs with her hands, and allowed the muscles to glide softly through her fingers. I cannot say whether these operations facilitated the circulation of the blood or whether they restored elasticity to the tired muscles, but their result was beneficial in the extreme, and our powers were fully restored."

In the *Gazette des Hôpitaux* for 1839, we read of the manner in which massage used to be employed in the island of Tonga. If anyone feels tired after exertion, he lies down and some of his ser-

vants practise upon him various operations which are known under the name of Toogi-Toogi, Mili, and Fota. The first of these consists in striking the various parts of the body with the closed fist, softly and rapidly, and corresponds to one of the methods of what we call *tapotement*. The second signifies the same thing as *effleurage*, which is thus described by Murrell :—" This is a stroking movement made with the palm of the hand, passing with various degrees of force over the surface centripetally. It may be practised with the fingers or with the thumb alone." The third is described in the *Gazette* as a squeezing and pressing of the skin with the fingers; this is identical with part of the manipulation now known as *pétrissage*. The *Gazette* further goes on to describe its use in fatigue, in headache, and in various painful affections to which the flesh is heir.

From the above short history, which could, were it necessary, be considerably extended, we see that massage, far from being a novelty which interested parties and enthusiasts would introduce into the practice of medicine and surgery, is in reality a "primitive institution coincident with the hygienic and therapeutic requirements of mankind," and has been employed at all times and among all the nations of the globe.

The question naturally arises, how it came to pass that a method of cure of such generally recognised efficacy should have fallen into disuse amongst members of our profession, and should have been for so long a time neglected? Dr. Bella Weiss, of Vienna, in an article in the *Wiener Klinik* for 1879, explains this extraordinary fact thus :—" In ancient times massage was employed by the most illustrious physicians. Hippocrates recommended its use in the most impressive language. It was an integral part of the exercises of the gymnasts, and amongst these were the most renowned doctors—Hippocrates, Diocles, Antyllios, Archigenes, Galen, &c. Massage was then held in honour, and deserved to be so; but as in the course of the degeneracy of morals it fell into the hands of slaves and courtesans, the physicians ceased by degrees to employ it, so that it became more and more a welcome source of profit to magicians, herds, and old women."

Many names might be mentioned in connection with its revival;

for in every country physicians and surgeons alike have recognised that this method of manipulating the body rests on a scientific and physiological basis. Mezger, of Amsterdam, has shown, by the fame of the results he obtains, of what massage is capable; and Professor Von Mosengeil by his experiments and researches has, perhaps, done more than any one man to remove from massage the stigma of charlatanism, and to place it in the ranks of scientific medicine; and I do not think that its advocates in this country need be ashamed of employing and testing a method of treatment which has received the sanction of such men as Billroth, Esmarch, Trousseau, Langenbeck, Weir Mitchell, and many others whom I might name.

To refuse to adopt and practise massage because for centuries its exponents were quacks and charlatans manifests the same spirit which animated surgeons not to touch a stiffened joint because "bone-setters" made fortunes by rupturing adhesions under the impression that they were reducing dislocations, and frequently cured when the surgeon had failed, and is as logical as it would be to refuse to prescribe a pill or a bottle because the public were treated by Holloway's Pills or Warner's Safe Cure.

The treatment by massage in former times was undoubtedly empiric. People discovered that its employment in health as a hygienic process was followed by sensations of well-being over the whole body. Hence it became an essential constituent in the baths of the antients. Even to-day it is practised in eastern countries with a thoroughness and perfection which is unknown in the so-called Turkish Baths in these countries. In "Letters on Egypt," an enthuasiastic writer describes how massage is employed in the Egyptian baths, and the exquisite sensations which the bather subsequently experiences:—"One feels a lightness and suppleness hitherto unknown; it appears as if one were born anew, and lived for the first time." Its use in therapeutics had a similar origin. It relieved pain, it effected cures in certain diseases, *therefore* it was widely employed by those whose duty it was to treat disease. But the practice now differs from then, chiefly in this—that medical men employing massage can give a reason for the faith that is in them.

Let us for a moment look at the process by which a limb is nourished. The material required to replace the waste of tissue continually going on in a limb is conveyed to it by the arteries, and when it enters the minute capillaries this nourishing material, the blood plasma, passes through its walls into the intercellular spaces around, thus bathing the various tissues with this nourishing fluid. In passing through the walls of the capillaries the plasma loses about one-half of its albumen and two-thirds of its fibrin, the other constituents remaining practically unaltered. Thus the quality of the nourishment supplied to the tissue is in direct proportion to the quality of the blood. In the tissues the plasma undergoes certain changes, due to the interchange which continuously goes on. It parts with those materials which the tissue of the part requires, and it takes up the waste material thrown off—that is, the products of combustion. This altered fluid then makes its way between the cells of the part till it reaches the lymphatic capillaries, and thence into the lymphatic vessels, whence it passes to the glands and through them, finally rejoining the blood-current through the thoracic duct. Its passage into the capillaries is chiefly due to the vis-à-tergo of the accumulating fluid behind it. Its passage through the lymphatic vessels is, in the main, brought about by the pressure exerted upon them by the contraction of the muscular fibres between which they pass. Later on the suction power of the right auricle comes into play. Now, the proper nourishment of a limb depends upon the efficiency and perfection of all these factors. It may fail because the blood is poor in its nourishing constituents; or because its circulation is so feeble that sufficient material is not supplied; or the return of the venous blood is slow or impeded, and thus checks the arterial supply; or the onward flow of the used-up plasma is sluggish. In health the quality of the blood is insured by the proper digestion and absorption of proper food. The circulation in the veins and lymphatics chiefly depends on the muscular contractions in exercise. But in disease these conditions do not hold. Take, for example, a broken leg. Rest is necessary for its cure—but this very rest at once abolishes those muscular contractions upon which the circulation in the veins and lymphatics mainly

depends. The arterial supply, at first, at any rate, is unimpaired. What is the result? Increased blood pressure at the part (I am not now taking the pathological condition of inflammation into consideration), and consequently an increased exudation of the blood plasma. Coincidently the lymphatic flow is checked, because the muscles no longer squeeze the fluid along the well-valved vessels, and œdema of the limb results. Moreover, the interchange between the material which is to build up the new tissues and the waste material—whether of natural combustion, or of inflammatory exudation, or of extravasated blood—is so materially interfered with that the process of repair is slow. And yet the ordinary treatment of a fracture by rest and splints does nothing to remedy this evil, and the only provision usually made to assist nature is to elevate the limb, and by gravity to replace the want of muscular contraction.

This is a surgical example, but medicine can furnish many on her own account. For the preservation of ideal health it is necessary to maintain an even balance among the various functions of the body. No more beautiful example of this is seen than that—whether in hot climates or in cold, whether in exercise or at rest—the temperature of the body is maintained at almost a stable quantity. The preservation of this equilibrium among the various functions is due to the controlling influence of the nervous system. Now take an example with which we are all familiar:—A girl for some reason or other is in poor health. Perhaps some violent shock of the nervous system is the cause. The effect is seen in an upsetting of the nervous control over this condition of equilibrium. The digestive organs are perhaps the ones to suffer. She is the subject perhaps of what Sir William Gull described in 1888 as *anorexia nervosa.* As a result the tissues waste and she gets thin. The muscles suffer—they are reduced in size and in quality, and are incapable of making the amount of exertion to which they were accustomed. The balance between repair and waste is lost. The tissues get overcharged with the products of combustion, and the quality of the blood itself is so impaired that even were the lymphatic circulation better than it is, still the plasma would not supply the nourishment required. The nervous system, in-

cluding the brain, suffers from this same want of supply, and its functions become more and more impeded. Thus the evil cycle goes on. Perhaps during the progress of the case that undefinable function of the nervous centres, which we call self-control, is lost, and the sufferings of the patient are increased by being dubbed hysterical, and consequently the location of her disease is at once transferred to a portion of her body which may or may not be entirely innocent of the state of her general health. How is such a case dealt with? The nerve centres, to recover their function, require absolute rest; and here again rest means loss of exercise and increasing anorexia. Here again the absence of muscular exercise implies a sluggishness of the lymphatic circulation, with overloading of the lymph spaces in the tissues, with accumulated waste products, and consequently an inability of the tissues to take up proper nourishment from the blood, even supposing that *it* were all that could be desired, which it is not. In the days before massage was re-introduced by the writings and teachings of Weir Mitchell, we tried tonics to the nervous system, and tonics generally, iron for the blood, delicacies to tempt the palate, artificially digested food for the stomach, and, when all these had failed, change of air either to the Continent or to another practitioner.

But now massage steps in to help us to keep our patients and to cure them. In the last example the nervous system gets its needed rest by keeping the patient strictly in bed, and as every muscular movement, and every mental excitement or disturbance, means so much waste of nervous force, which we are endeavouring to store up, we forbid all movement, and we remove her from home surroundings and visitors, and everything which can possibly supply her with food for thought, which is generally synonymous with food for anxiety or worry. In the next place we massage her. Time will not permit me to describe the method of carrying out these various manipulations. They are best learnt by a little practical experience. When I first commenced to employ massage, I began by learning to do all the manipulations myself, and I have ever since found it of the greatest practical use. For though it is impossible for a medical man to massage his own cases, and most frequently

inexpedient, still, in certain cases requiring only a local and limited massage, he should be able to do it when necessary, as I have on more than one occasion experienced. But in the majority of cases, which must be handed over for manipulation to a well-trained nurse or masseur, it is of inestimable value to be able to see, as it is being done, that it is correctly done, and, if faulty, to show how to do it. Suffice it here to say that, beginning at the toes and working steadily upwards, first the skin and, secondly, the muscles and deeper structures are submitted to a process of gentle pinching, kneading, and percussion, until the whole body has passed through the hands of the operator. In cases of general massage, such as the one under consideration, I prefer to employ it twice a day. I have found that an afternoon's massage is more efficacious than when this time is used in applying electricity, as recommended by Weir Mitchell. In some of the earlier cases I tried massage in the forenoon and electricity in the afternoon, but I have obtained better results by dispensing with electricity and employing massage alone both in the forenoon and afternoon. The morning *séance* occupies about two hours; the one in the afternoon about an hour and a half. I think it scarcely possible to go over the whole body in an ordinary sized adult in a shorter time. Some patients, especially neurotics, complain of a certain amount of soreness and pain, but this disappears in the course of a few days. Others like, or at any rate do not mind, the process from the start. I have myself undergone massage of one arm and shoulder and the side of the neck every day for a fortnight for rheumatism, which baffled all other attempts at cure, and I can say that if, under skilful hands, pain is elicited by the process, it is evidence of a condition of hyperæsthesia. The manipulations must, in every case, be at first extremely gentle, but it is astonishing to see the amount of force which can be used in a short time without producing any discomfort on the part of the patient.

But to return to our hypothetical case. Massage is employed twice a day. By proper kneading of the muscles and other tissues the accumulated waste materials are, in the first instance, as it were, disentangled from the cellular network among which they lie

stagnant; and the continuous kneading upwards forces the exuded plasma, with the waste material it contains, from the lymph spaces into the lymph capillaries, and so on into the lymphatic vessels. At the same time the same manipulations force the venous current towards the heart, so that all the advantages of active exercise are obtained, together with absolute rest. At the same time the circulation in the various regions is stimulated, as is seen by the increased redness of the part massaged; the interchange between the blood plasma and the tissues is not only re-established, but is stimulated into a condition of activity which, I believe, is unattainable by any other means. As evidence of this we find the temperature is nearly always raised. If we take in such a case the temperature in the axilla before and about half an hour after massage, we generally find an increase of about half a degree to a degree. In a few instances the converse is the case, and though this is looked upon by some as an unfavourable symptom, I have not always found it so. I have recently had under my care a young lady suffering from anorexia nervosa, complicated by persistent hysterical vomiting, who presented the following symptoms:—Great emaciation, waxy appearance of the skin of the face, with dark purplish red patches on the cheeks, and a bluish nose. The hands were always cold, with bluish tips and sluggish circulation. The hands and arms used to "die" every morning up to the elbows. She had been treated by tonics, change of air, &c., without the slightest benefit. During the first ten days of the massage treatment the axillary temperature after massage was almost invariably lower than before it. Nevertheless she has made a perfect recovery; she put on two stones in weight during the six weeks; and her colour is as good as could be wished. The vomiting has ceased and she eats plentifully. Her hands have resumed the normal appearance of health, are always warm, and never "die" now.

The result of the rapid interchange in the tissues is that the nourishing plasma of the blood becomes rapidly used up. This must be replaced by nourishment. When such a case as we have been considering first comes under treatment we usually find a

foul tongue, with constipation and a loathing of all food. To remedy the constipation Weir Mitchell advises the use of an aloetic pill. I prefer cascara sagrada; and although you may at first be told that the strongest purgatives are of no use, I have not yet met a case which resisted the abdominal massaging assisted by cascara. During the first few days—sometimes for the first week—I have invariably followed Weir Mitchell's advice to restrict the diet to small quantities of milk given every two hours. Usually the tongue cleans rapidly and the patient complains of being starved. The diet may then be gradually increased, until about the end of the second week, in most cases, the quantity consumed would do credit, as Playfair says, to a life-guardsman. In a few cases I have found that the tongue refused to clean on the limited and fluid diet; a little solid food, such as an egg and toast at breakfast, and a chop in the middle of the day, has speedily brought about the desired result. I once had a case which made a great impression upon me. The patient had been a fine healthy girl, full of life and spirits, but as a result of a great deal of trouble and anxiety she broke down in health, became desponding and gloomy, and was continually "fainting." From being a girl remarkable for her powers of self-restraint, she became emotional and hysterical. Her general health suffered simultaneously. She lost her appetite, and her complexion was waxy. I began a regular course of Weir Mitchellism on her on the 15th of December, 1886, in lodgings in Dublin; but in spite of all I could do I could not get the tongue to clean, and she continually complained of pain in the epigastrium and sickness. It then occurred to me that the lodgings were in an unsanitary condition, and as she was making no progress, and as the attendance was very bad, at my suggestion she changed them, at the end of four weeks. I then began again at the beginning, and was gratified to find that in a few days the pain and sickness left her, the tongue cleaned, and at the end of six weeks, in the new lodgings, the cure was complete. The ultimate result was all that could be desired, but the treatment occupied in all ten weeks.

In this connection I will mention another case, because the

President of this Section of the Academy knew something of the case and very kindly advised the girl's mother to place her under my care. She was aged seventeen, but was so emaciated that she only weighed a little over five stone. She suffered from anorexia of the nervous type, and constipation was said to be absolute. I began the massage treatment on the 30th of November, 1886. At the end of three or four weeks the bowels acted daily without medicine of any sort, and by the end of the course she had gained over two stone in weight, her face had become rounded and healthy, the body was well nourished and plump, and the ribs, which could have been counted across a room, were hidden by the overlying fat. She has remained in perfect health ever since.

The cases I have alluded to belong to a large class which, for want of a better name, are usually called neurasthenics. We find them among both sexes, though more commonly in women. They all have certain symptoms in common, but they might well be divided into classes, according to the prominence of certain symptoms over others. Thus in the cases I have quoted anorexia and wasting were present without any defined lesion to account for the condition. In others, neural pains are the prominent feature, and the relief which many of these cases experience from massage illustrates the truth of the saying of Sir Thomas Watson, that neuralgia is the cry of the nerves for pure blood. Plutarch mentions how Cæsar was cured of neuralgia by rubbing. One of the worst cases of this kind which I ever had to deal with came under my care in October, 1884. The patient was a married lady, aged forty-two, with the following history, which I give in a condensed form:—Fourteen years previously she had been attacked with violent pain in the back, which was called lumbago. Four years later she had a similar attack, whilst out riding, which was so violent that she had to be lifted off her horse and left on the roadside until a carriage could be sent for her. Since that day she had always had more or less uneasiness, but it was not till 1883 that the pains returned in an aggravated form. From November, 1883, until I saw her the following October she had been obliged to stay most of her time in bed, being occasionally helped on to

the sofa. The pain at first was down the course of the right sciatic nerve, but after a time she had pain also in the left leg and in her back. When I saw her first she was unable to move out of the bed without assistance; she was rather over than under nourished, but the surface was everywhere waxen in colour and appearance, she was markedly anæmic, and she presented the other usual symptoms of neurasthenia. On examination I found that the right leg when extended could not be raised off the bed more than two or three inches without causing excruciating pain, while the left could be raised to nearly a right angle to the trunk. I have found this a most useful means of diagnosticating a sciatica the result of former inflammatory mischief in the sheath from that form which seems to be a pure neuralgia. On November 1, 1884, she was placed under ether by Dr. Wallace Beatty, and I then forcibly stretched the right sciatic by Billroth's method—that is, with the foot flexed on the leg, and the leg fully extended, the thigh is flexed to about a right angle with the trunk. By this means the nerve is stretched round the great trochanter. This was followed by almost complete relief as regarded the right leg, but the neurasthenic condition continued and the ill-defined pains elsewhere were unabated. She made no progress whatever towards recovery. I then advised massage, and after some little difficulty consent was obtained, and the manipulations were begun on the 13th of the following January. As advised by Weir Mitchell in fat anæmic cases, I kept her on very scanty diet—chiefly skimmed milk—for nearly three weeks, and then began to feed her up well. The treatment lasted for nearly ten weeks. At the end of it all the pains had left her, and she was able to attend to her household duties as she had not been able to do for years. She has practically been in perfect health ever since.

In October, 1887, I first saw a young lady whom I was told was suffering from the results of a sprained ankle. She had sprained it sometime in the summer, and had been under treatment for it ever since. When I saw her she could not put her foot to the ground, but went about on crutches. She could not bear the slightest touch to the foot; but, after the most careful examination,

I could not find anything the matter with it. At the same time I observed that she presented a well-marked series of neurasthenic symptoms—anorexia, wasting, anæmia, &c. I therefore advised general massage, which I began in the house of a friend of the patient's, which was otherwise unoccupied, and where she was completely isolated. At the end of six weeks she was allowed up and had apparently forgotten all about her crutches. The improvement in her general health was most marked, and has continued so ever since.

Another symptom, which sometimes is the prevailing one, is insomnia. Gentle massage of the spinal region, which is useful in simple cases of sleeplessness, is unavailing when the insomnia is only a complication of general neurasthenia. The most rapid and most permanent cures in such cases are, I believe, to be found in a thorough course of massage. In 1880 I attended a clergyman who had suffered off and on for years from insomnia of an aggravated kind, accompanied by great mental depression. He had on former occasions been treated with opium and chloral, but without effect. The only thing which did him any good was going abroad. I prescribed bromides for him in various doses and in various ways—by themselves, or combined with other drugs—but they did him no good, and only increased the depression. I then tried Turkish baths, and was surprised to find that if he took a bath he usually had a good night after it. He improved very much up to a certain point, but then the baths began to lose their effect, so I advised him to go abroad. This for the time sufficiently restored him to enable him to resume his duties in the country. For several years subsequently he had returns of the depression and insomnia in a milder form, which were generally remedied by a holiday, or going abroad, or taking Turkish baths. In 1886 he again had a severe attack, for which he came to Dublin and tried the baths again, but with very little result; he returned again to the country, but got so much worse that his family were afraid of leaving him alone. I then advised a course of general massage, but with a certain amount of misgiving, as I was not sure whether his mental condition were purely functional, or whether there might not be some

more serious organic mischief underlying it. Still it seemed to me at the time that to leave him in his then condition might lead to the most serious ultimate consequences, and drugs had proved more harmful than beneficial. Accordingly, I began the treatment on November 8, 1886. It was, perhaps, the most difficult and troublesome case I ever undertook. The course lasted seven weeks, and at the end of it he was so much improved that a sleepless night was exceptional. However, the benefit did not stop with the massage. I advised him to go abroad at once, which he did, but returned unexpectedly in less than a fortnight and resumed his work. A few months later I heard that he was in wonderful health and slept well. He has had no return of the attacks of insomnia and depression since; but I hear that he attributes his recovery to the foreign trip and not to the massage. A remarkable feature in this case was, that even when at other times most wakeful, he frequently fell asleep while the manipulations were going on, and sometimes slept right through the whole proceeding.

There are other conditions besides neurasthenia in which general massage may be of the greatest use. I have tried it in different paralytic affections with varying results. I can say that I have never seen it do any harm in any of these cases; but I have had some in which I could not honestly say that it did the paralytic condition any good; but in some of the cases the result exceeded my most sanguine expectations. In acute anterior polio-myelitis the benefit to be derived from this method of treatment is fully recognised. I had one such case under my care in hospital. The child had been under the care of one of my colleagues during the acute stage of the attack, during which time he had been treated by various medicines. Owing to the absence of my colleague from town he was handed over to me. I found that he was almost completely paralysed from the neck downwards. He could not move a limb, and he could only move the head with difficulty. I had him systematically massaged twice a day, and I ultimately had the satisfaction of discharging him perfectly cured. Of course it may be said that this result might have come about without the employment of massage at all; but this expectant method of treat-

ment most frequently fails, and, in the meantime, the occasion in which massage may do good will have passed away.

The cases, however, of infantile paralysis which have most frequently come under my notice have been cases which have existed for years—cases in which we find an undeveloped, cold, and wasted limb, a mere appendage to the rest of the body, and of not the slightest use. I have had a few such cases in which the upper extremity had suffered, and many more in which it was one of the lower limbs. What can we reasonably expect to accomplish in such a case? Cure is out of the question. Under prolonged, patient, and skilled massage I have seen the wasted muscles grow, and the limb recover somewhat of its warmth, and the flail-like condition of the extremity to a certain extent diminish; but more than this massage cannot effect.

In other paralytic and allied affections, depending on a recognised central lesion, we cannot expect to do much with massage—such as true paraplegia, hemiplegia, progressive muscular atrophy, tabes dorsalis, multiple sclerosis, and the like. But there are many cases in which the paralysis is as true as in any of the above, but in which the central lesion is uncertain. We call them functional cases until our therapeutic *repertoire* is exhausted, and then we decide that there is an organic lesion. But who can draw the sharp line dividing the one class from the other? My colleague, Dr. Heuston, treated successfully a case by massage, in the Adelaide Hospital, which had resisted the efforts of many and eminent physicians for sixteen years. I had a somewhat similar case under my care in 1886–87. A young lady, who was staying on a visit with some friends in Cyprus in 1885, was attacked on the 19th of November with what the doctor who attended her called "malarial typhoid fever," when the temperature rose to 104°, but it subsequently rose much higher. "At the end of the sixth week," writes the doctor, "the patient suddenly collapsed, and temperature fell from 103° to 96°. She appeared to be in articulo mortis, but the pulse continued regular, though very weak; and by dint of stimulants and nourishment she rallied, but on the slightest movement or noise became insensible; heart still working regularly, but coma

continuing, sometimes up to two hours, when the heart intermitted and the symptoms became alarming. From this time she had paraplegia from the hips downwards, which has continued. She made some slow improvement from this time, but one day in trying to lie on her side the legs fell out of bed and dragged her body after them. She was found in a sitting posture on the floor, and on being got into bed became comatose as usual."

She was eventually brought home in a steamer to London, and thence by sea to Dublin, being scrupulously kept in a horizontal position. I saw her for the first time after her return on the 14th of September, 1886. I then learnt that during her illness, in order to keep her alive, the foot of the bed had been raised, so as to keep the blood circulating in her brain, for six weeks. It was not until the end of this period that the paraplegia was discovered. On examination I found that both legs were paralysed completely from the hips down. She was absolutely incapable of making the slightest movement; the limbs lay like logs. Sensation was also completely destroyed in both—pins could be stuck into any part of them without her being conscious of it. Both limbs were wasted and thickly covered with hair. She was seen in consultation both by Dr. Head and Dr. Wallace Beatty. The only symptom which suggested that the paralytic condition might be functional was that the upper limit of the loss of sensation was ill-defined. We agreed that it would do no harm to try massage. Accordingly a skilled masseuse manipulated the whole body for about six weeks, and then taught the patient's sister how to do it. The latter continued to massage the limbs alone twice a day for many months. It was most interesting to watch how, after months of patient work, power gradually returned—first in the toes, then in the foot, and finally throughout the entire of both legs. Simultaneously sensation also returned, and the hairs gradually disappeared from the surface. About twelve months after I first saw her she was able to walk across town to see me.

There are many other conditions affecting the system generally in which massage is often of great benefit, but which I cannot allude to now. But I would like to say a word about its use in

gout. In elderly gentlemen, even during the acute stage, I have found it beneficial. In these cases the method must be somewhat modified. The procedure I have used when the acute stage is over is to keep the patient in bed for breakfast, shortly after which he is carefully massaged for about two hours. Then he lies quietly in bed for an hour, during which time he often sleeps. About half-past twelve or one he gets up and dresses for lunch, after which he goes out for a drive, should the weather permit, and then returns to dinner and goes to bed early. Four to six weeks of this treatment I have found in several cases has procured an immunity from a return of the disease for long intervals of time—much longer than they were previously permitted to enjoy.

If we now turn to the application of massage to surgical cases—a portion of the subject which in this Section I shall touch on but very briefly—we shall see that for the most part we have to do with local massage; we wish to bring the effect of massage to bear upon one particular part; general massage, which has such a beneficial effect on the general system, is but rarely required. As an instance, however, of a surgical case in which it may prove useful, I may mention that of a young man who came into the Adelaide Hospital with advanced disease of the knee-joint. An attempt was made to save the limb by practising an excision, but the disease returned in the ends of the bones and made such rapid progress that it became evident that the leg would have to be amputated. He was, however, in such an exhausted state from pain, discharge, and loss of appetite, that I feared to expose him to the risks of amputation. It occurred to me to try the effect of massage, and that this was a case to test in practice what I believed in theory. The limb was swung in a Salter's swing, and the body was thoroughly and completely massaged for two hours a day, the diseased leg being untouched. It was very remarkable to notice the rapidity with which he gained ground. His appetite came back, his colour improved, and he began to put on flesh. As soon as I thought he had gained sufficiently in strength I amputated his leg above the knee, and when he left hospital no one would have believed from his appearance that he had undergone so serious

an operation, which, had it been attempted before massage was employed, I believe must have ended fatally.

The surgical cases in which I have employed it—and generally with remarkable results—are sprains and fractures, and with a view to preventing the re-formation of adhesions in a stiff joint in which the adhesions have been broken down.

In recent sprains and fractures we have not only to stimulate the circulation in the part and to aid the onward flow of the waste materials, but our object is to cause the absorption of extravasated blood and of inflammatory exudation. I have heard it objected that a badly sprained ankle and a recent fracture will not bear the pain of their being rubbed and manipulated. There is unquestionably rubbing and rubbing—and the difference to be found between the manipulations of a skilled hand and those of the so-called "rubbers" is as marked as between a master's work of art and a daub of paint. My own experience of the effect of massage on sprains corroborates in every particular the description given of it by Mr. Mansell Moullin in his work on "Sprains." "In recent cases," he says, "the greatest care is required, and nothing is so likely to increase the mischief as rough handling of the part; but, when it is carried out quietly and gently by one who has had some experience, it is very difficult to find anything that acts in so perfect a manner. The whole limb, perhaps, is swollen, the joint distended with blood, the skin shining and tense, much too hot to the touch, and exquisitely tender—but all this vanishes as if by magic. The tension disappears as the fluid is carried off, the pain is relieved, the temperature falls, the natural outline begins to appear once more, extravasated blood is broken up, the *débris* dispersed, and adhesions between the torn and bruised surfaces effectually prevented." In the Prussian army, where treatment by massage is in certain cases obligatory, it is found that the average duration of the disability from sprains not treated by massage is twenty-seven days; whilst when massage is resorted to it is only nine days. In fractures the results are equally surprising. The method I have employed is usually this:—The limb is placed in a splint conveniently arranged, so that with the least

amount of disturbance it can be removed. The limb is massaged gently but firmly up to the seat of fracture, or near it; and from this again up to the trunk. The parts implicated in the fracture are massaged with the greatest possible care and gentleness, and pain is rarely complained of. This is done twice a day, and occupies about twenty minutes to half an hour each time. The splints are then readjusted.

The following examples, I think, are worth recording here:—

A valet, aged thirty-two, fell on the 20th of September last and sustained a Potts' fracture of the left side. The foot was dislocated outwards and the internal lateral ligament was completely torn. He was put up by Dupuytren's method, and sent up to hospital on the 3rd of October, thirteen days after the receipt of the injury. On the 8th of October massage was begun. The swelling and ecchymosis, which had been extensive, rapidly subsided; and on the 25th of the same month he was able to put his foot to the ground and to bear some weight on the limb. On the 30th he could walk perfectly. The joint was freely movable and quite painless. He left the hospital on November 4th. In this case massage was not begun until eighteen days had elapsed, or the result might have been even more rapid; but, as it was, the complete restoration of the parts, so that he could walk painlessly and well in forty days from the time of the accident, is a result which is, I believe, unattainable by any other method of treatment.

The last case to which I shall refer is that of the boy, aged fifteen, whom I have had an opportunity of showing to the Academy. On the 16th of March last he sustained a transverse fracture of both bones of the leg by falling against a plank of wood when running. The limb was set and placed in splints by Dr. Davy, of Terenure, and the same evening he was admitted to the Adelaide Hospital. On the 17th of March I examined him and found a transverse fracture of the upper third of the left tibia. Crepitus was distinct; there was a good deal of swelling and ecchymosis, but there was little or no displacement. The leg was put into a box splint, so as to be easily exposed, and massage was begun the next day. On the 8th of April—that is, one day over three weeks—he was able

to raise the leg from the bed, and when helped on to the floor could walk with assistance and with very little pain. His condition to-day—the twenty-sixth day since he broke his leg—you have been able to see and judge for yourselves.[a]

The length of this communication has far exceeded what I originally intended. I have endeavoured to condense it as much as possible, and to quote only those cases which illustrated some special point of more that ordinary interest. I have scarcely at all referred to failures and disappointments, although I have met with such. It is hardly possible to avoid failures and errors when trying a method of treatment which is still in the infancy of its revival, and I believe I have learnt almost as much from the failures as from the successes. I may, however, say this, that the failures have chiefly been in those cases in which I tried massage as a tentative measure, and of the results of which I felt doubtful; but the number of failures has been exceedingly small. I have employed massage in sixty cases, exclusive of hospital practice. Most satisfactory results were obtained in thirty-nine—some of these being truly astonishing. In fifteen cases there was decided improvement; in three there was distinct failure; and in one the treatment was abandoned a few days after it was commenced on account of an attack of epidemic dysentery, of which the patient died. Two of the cases are too recent to classify. The three failures were—one case of true paraplegia, in which the diagnosis was doubtful; one case of progressive muscular atrophy, in which I tried it tentatively; and one case of locomotor ataxy, in which the treatment failed to do good, except to relieve constipation, with which the patient was troubled.

I think these results warrant me in saying that we have in massage a potent therapeutic agent, and one which merits well to be more generally employed.

[a] The patient walked into the room after the reading of the paper, without assistance, and without any appreciable limp.

ACUTE CONFUSIONAL INSANITY.

By CONOLLY NORMAN,
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[Read in the Section of Medicine, May 16th, 1890.]

THE form of insanity to which I wish very briefly to call attention has not, so far as I am aware, met with recognition in this country hitherto. This is not due to its rarity, however, as there can be no doubt of the truth of Salgó's dictum, that acute confusion is the most common of all forms of insanity, even though we should not accept in full the conditions which make this author's definition of the state somewhat wider than that which we ourselves admit.

If it were necessary to indicate in the shortest and most generally comprehensible way the relations of this affection to the states of alienation usually recognised under the names applied by Pinel, one would say that acute confusional insanity stood between acute mania and acute primary dementia. This statement of its position will at once show to those familiar with the treatment of early cases of insanity in the forms ordinarily met with in general practice how large are its contents, for cases occupying this intermediate ground must have struck everyone by their frequency, and by the difficulty of satisfactorily denominating them under the older headings.

Acute confusional insanity may be described as a condition of mental disturbance of comparatively rapid oncome, characterised by a dream-like engagement of consciousness, and a tendency to abundant hallucinations of one or more senses.

According as the confusion or the hallucinations predominate does any individual case resemble acute dementia or mania (melancholia). Predominance of confusion corresponds to the delusional stupor of Newington, and where hallucinations give the prevailing

tone the condition is that which Mendel has named hallucinatory mania.

In average cases I have not found hallucinations of the senses so very prominent a symptom as some authorities have taught, and I am glad to find that Meynert, to whom is due the merit of having first clearly and comprehensively differentiated this state, has in his latest contribution discarded the term which he originally used, acute hallucinatory insanity (Wahnsinn), and has adopted the term confusion.[a]

The condition under consideration is almost always acute in onset; in form it is occasionally acute or peracute, more generally subacute. True chronicity can hardly be said to exist, but uncured cases lapse into secondary dementia probably earlier than cases of mania or melancholia.

The disease, when its beginning can be distinctly dated, usually makes its appearance by the occurrence of hallucination. A certain degree of dreamy obscuration of the mind has preceded this stage, as we often find from the statements of recovered patients, but this frequently escapes attention. When the condition is fully developed, consciousness is profoundly engaged. The patient has lost his sense of orientation and his knowledge of his surroundings, or, if he can be roused to correctness on those points, he soon drifts back into the obscure condition. His estimate of time is entirely confused. He dates events of yesterday as having taken place a week ago, of a week ago as being six months old, and so forth. He does not lose his sense of individual personality, nor build up an organised system of delusion like the paranoiac. Varying and disconnected delusions flit through his mind, and are temporarily accepted as we accept the truth of dreams. The contents of the dream may be pleasant or the reverse; the hallucinations may seem of an agreeable character, or may be threatening or awful;

[a] I am by no means ready to say that the name I myself use, and which I have put at the head of this paper, is a very satisfactory one. Our language does not readily lend itself to minute subdivisions of mental states, either sound or morbid, and I have not ventured to dig for Greek roots, or add another to the puzzling and somewhat barbarous seven-league words of which we have in our science already a great deal too many.

and thereafter, to a large extent, follows the emotional state of the patient. I think in the majority of cases the mental contents are not decidedly tinctured with either pleasure or pain, hence the emotional state is commonly indifferent; but it may exhibit considerable exaltation, or, what is much more common, considerable depression. Two features strongly distinguish the emotional condition in confusional insanity from that in mania or melancholia. First, it is variable. The patients are, as Wille briefly says, sometimes gay, sometimes sad, sometimes anxious, sometimes angry, sometimes tender, or all these things together, or in the most rapid succession. In short, confusion reigns in the emotional as in the intellectual sphere. Secondly, the emotional disturbance is a reactive one, arising from the nature of the hallucinations. The reverse holds good in melancholia and in mania.

The patient's acts, as well as his feelings, are dictated by hallucination. He responds to sensory hallucinations even more readily than the paranoiac, but, of course, his conduct does not so clearly exhibit his state, owing to his dreamy confusion and the varying and unsystematised nature of his hallucinations.

Episodic reactive states of emotional excitement or motor restlessness are apt to be suddenly followed by periods of increased confusion, deepening into stupor, or stuporous conditions intervening directly.

The contention of von Krafft-Ebing, that acute confusional insanity is essentially a condition of brain exhaustion, and probably due to anæmia or malnutrition of the cortex, appears to be well founded. The phenomena of the affection suggest this view, which is strictly in accordance with the histories and with the physical aspect of our cases generally. The patient is usually feeble and anæmic, and in very many instances suffers, or has recently suffered, from some exhausting disease.

This is more often than any other the form of psychical disorder which is associated with diseases not primarily affecting the nervous centres. Puerperal insanity commonly takes this form. The insanity of rheumatism is usually also acute confusional insanity. So also is the insanity that follows fevers, and it is interesting to

note that occasionally the delirium of fevers passes directly into acute confusion. The latter condition, it will be observed, bears a considerable resemblance to the former. Prolonged lactation, chronic suppurative affections, diseases of the stomach and of the lungs, notably phthisis, have a strong predisposing, if not exciting, influence. Von Krafft-Ebbing describes this form as occurring in cases of insanity arising in prisoners. I have seen one such case, but insane prisoners whom I have happened to see were more frequently sufferers from either acute mania or from paranoia.

I have seen several cases which appeared to be associated with nostalgia. Simple folk who had come from distant country places to a large town became, after a short period of unrest, troubled with acute hallucinations of threatening contents, and rapidly fell into extreme confusion. It is only right to say that in some of these cases there were bodily ailments for which the patients had to come to town for treatment, in others there was the history of a drinking bout; but in several there was neither of these factors, and the only assignable cause was nostalgia, together with unsettlement of mind and habits, produced by altered mode of life.

Acute confusional insanity, generally with hallucinations as a very marked trait, commonly occurs in cases of sexual excess or irregularity.

I have seen one very painful case in which this form of alienation followed rapidly on a painful mental shock. It is instructive to note in connection herewith that the most common form which insanity takes when it follows sudden shock is the kindred one of acute dementia.

It has for some time seemed to me a singular thing that a very well-marked form in which insanity following alcoholic excess constantly appears has attracted so little attention. The term *mania a potû* is used often enough, but no definite descriptive sense has been attached, and mostly people are content with the true but somewhat indefinite generalisation that it is something between *delirium tremens* and acute mania. Nevertheless, as I have been in the habit of teaching my classes for the last two years, a certain train of symptoms is almost always found in

acute insanity from drink. These symptoms form an exquisite picture of acute confusional insanity. There is in a very marked degree loss of orientation, and a sort of dream-like impairment of consciousness, with numerous hallucinations. According to my observations, dreamy confusion is a more prominent symptom in female alcoholists, and hallucinations in men, but both are present in such cases in varying degrees. The association of a peculiar form of confusion with alcoholism has not altogether escaped notice. In a paper read at a meeting of the Medico-Psychological Association, at Manchester, on the 13th of March, 1890, Dr. James Ross describes an intense confusion as to dates and events as characteristic of the dementia accompanying alcoholic neuritis. In the debate arising thereon, Dr. Wiglesworth confirmed Dr. Ross's observation, and I find that Dr. Korsakoff, of Moscow, described, in 1887, in connection with alcoholic neuritis, a "form of confusion with extremely characteristic mistakes in relation to place, time, and situation."

In the last number of *Westphal's Archiv*, Korsakoff describes a number of cases presenting this particular form of confusion in a very marked degree, associated with peripheral neuritis of non-alcoholic origin.

The course of this affection is very variable. The onset, as has been said, is often acute. I think insanity which is described by the patient's friends as having come on "out of sleep," is always of this type. In such cases a vivid dream appears to be accepted as true and followed by a brisk reaction. Thus acute confusional insanity is brought into line with that state occasionally present in the sane, and especially in those of neurotic tendency and in epileptics, which has been called *Schlaftrunkenheit* by German authors. The duration may be very short, lasting only a few days, or even, in abortive cases, only a few hours—*e.g.*, some cases associated with menstrual disturbance, as von Krafft-Ebing correctly points out. The last-named writer calculates his recoveries as amounting to about 70 per cent. Cases that are about to recover occasionally pass into a state resembling acute mania. Meynert, who first observed this occurrence, thinks that the functional

hyperæmia accompanying the maniacal attack brings about a tendency to cure by increasing the circulation of blood through the exhausted brain. More common as an indication of recovery is a slight degree of stupor resembling that through which the patient, convalescing from an attack of acute mania, so generally passes. Prolonged periods of stupor, resembling, and probably identical with, that occurring in acute dementia, occasionally precede recovery. Less favourable signs are a mixture of maniacal and stuporous conditions, or a tendency towards pathetic and histrionic displays, or the occurrence of pseudo-tetanic or pseudo-cataleptic states. The latter symptoms, when accompanied by verbigeration, constitute a close approximation to katatonia, which is indeed probably only to be regarded as a variety of the general affection under consideration. As in all cases of acute insanity, death from exhaustion may occur in an early stage, and there is in the usually debilitated sufferers from this disease a special tendency to succumb to intercurrent affections.

The diagnosis of acute confusional insanity lies in the distinctions to be found between this state and the allied conditions of acute mania, acute melancholia, and acute dementia; also certain forms of paranoia. From acute mania it is distinguished by the absence of exaltation and of increased rapidity of thought and association. I am not inclined to go with Salgó and say that any case in which hallucinations occur must be rejected from the denomination of mania, though hallucinations are much more characteristic of the affection under consideration. True emotional depression as a primary symptom is absent in acute confusion, whereby the latter is distinguished from melancholia. It is very intimately associated with acute dementia, and it is not always possible to say which form we are dealing with, though the presence of hallucinations and the absence of complete stupor in a typical case of acute confusion sufficiently denote the ailment.

From paranoia it is distinguished by the want of systematisation in delusion, by the existence of confusion, and by the sudden mode of oncome.

The following are brief abstracts of some cases which have

occurred in my practice chiefly within the last two or three years, and I have selected them because they are typical of a number of similar cases noted within the same period:—

Case I.—*Acute Confusion, associated with Alcoholic Excess; Neuritic Pains; Recovery.*—X. Y., female, aged about forty; widow of a merchant-tailor; hereditary history could not be ascertained; a sister is an habitual drunkard. Patient was supposed not to be of as high a social rank as her husband, consequently his friends would not know her, and she led rather a solitary life. Drank steadily, at first on the sly, afterwards more unrestrainedly. In the year 1884 had an attack of insanity, said to be brought on by drink. Admitted under my care, December 28th, 1888, four or five days after her husband's death. On admission she presented the ordinary appearance of the alcoholist—a bloated, swollen-looking face, darkly flushed, with greasy skin, staring eyes, injected conjunctivæ, and a tremulous, jerky, coated tongue; general shakiness, of the *delirium tremens* type; manner dazed; she seemed to make an effort to rouse herself to reply to questions, and talked slowly, and with much confusion; she talked of her husband's long illness, which she could neither date nor describe; spoke vaguely of continual quarrels with his nurse and his relations, but could give no details, or confused different individuals and different times in a way that made the story entirely unintelligible; she confused the dates of her husband's death and funeral; sometimes she said he died last Monday and was buried on Thursday, sometimes he died the previous Thursday and was buried last Monday; one event occurred on a Monday and one on a Thursday, but which on which day she could not be sure.

December 30.—Told my colleague (Dr. Cope) that she saw her sister, and heard her voice asking for the children. This troubled her, for she thought her sister was dead. Dull, dazed, sluggish, silent, unless when spoken to. Complained of pains shooting through extremities, particularly backs of hands and feet.

January 3.—Called the charge nurse "Mrs. B." (her husband's nurse), and asked her for "the keys" to get some whisky from the cupboard. When she was questioned about this she called the nurse by the proper name, and knew where she was; but again, after a while, called her Mrs. B., and spoke to her as if she was at home.

January 5.—Told Dr. Cope that her younger son, Tom, ten years

old, slept in her bed last night; he was ill and restless. Later on she told me this must have been a dream.

January 7.—Sluggish, dreamy, and unintelligent; *sure* both her little boys are dead; pains shooting down her arms and legs, and constantly present in dorsal surfaces of hands and feet; knee jerk equal, diminished; all the muscles flabby and feeble; no distinct paralysis; no drop wrist or drop ankle.

January 21.—"Gaining intelligence of manner and expression; still puzzling about her children and sister whether they are living or dead (as a matter of fact they are all alive)."

From date of last note there was steady improvement. Her recollection of the events preceding admission remained vague, summary, and confused, but she was conscious of this confusion. She stated that her previous illness had been exactly of the same character. She continued to suffer from pains—vague, shifting, darting pains—as if in the bones or deep in the muscles, chiefly in legs and forearms, and especially hands and feet. Discharged, recovered, April 3, 1889.

Case II.—*Acute Hallucinatory Confusion associated with Alcoholic Excess; Epileptiform Seizures; Recovery.*—The above is an extremely typical case of alcoholism in a woman. While she was under treatment an almost identical case was admitted into the Asylum. The details are so similar that I need not trouble you by entering into them. A woman of nearly forty, mother of two children, had a strong hereditary tendency to drink, to which she had entirely given way. She was deserted by her husband, and lived with relations who were always drunk. Had suffered from occasional epileptic fits. When admitted (February 20, 1889) she presented all the appearances of recent hard drinking; next morning she had two epileptic or epileptiform seizures; mentally she presented a state of confusion such as I have above described; she was slow and dreamy in conversation; could be roused to comparative clearness, but soon became confused again; thus she mistook me for the doctor who had been attending her in a suburb; the error was pointed out; she acknowledged it, but in talking to me drifted into references to my previous visits to her, and so forth, showing that the confusion persisted. After initial restlessness of the *delirium tremens* type had passed off she became sluggish. Four days after admission she rushed, shrieking, to a window, and broke several panes of glass. When I asked her next day how her wrists were

cut, she replied that she had been in the waiting room at a railway station, and that two men in green velvet had seized her, and would have robbed her but that she broke a window and attracted notice. She pointed out the nurses who brought her from the window as a woman and a policeman who had come to her rescue. About four and twenty hours later, when reminded of these statements she said that she must have been dreaming, that she remembered these things as one recalls a dream. After this she made rapid progress to recovery, and was removed by her friends, apparently quite cured, March 8.

Case III.—*Acute Hallucinatory Confusion associated with Alcoholic Excess, &c.*—I have at present under treatment a young woman, twenty years of age, married a year and a half, brought up by drunken relatives, who deny hereditary taint, and state that the patient drank to excess since her marriage, but not before; childbirth six months before admission; child suckled for four months; menstruation has not returned. Fourteen days before admission, patient went to England with her husband, an artisan, who was looking for work; six days later was brought back to Dublin in a state of mental disturbance. When admitted she was extremely anæmic, though without other distinct sign of physical disease; she did not know the day of the week or the month, nor where she was; said she was not married, and never had a child, and always gave her maiden appellation when asked her name; when one spoke to her of her husband she looked blank, and did not seem to understand; when one mentioned his name, Thomas E., she said, "Is that old Tom E. that lived next door?" She was confused, but quite calm, without trace of either exaltation or depression. One dark night, about 12 o'clock, she got up, and broke the window of her bedroom. On inquiry, she stated that she did this because she saw another old neighbour of hers, "Lame M.," walking past, and wanted to attract his notice.

Case IV.—*Confusion in the special form described by Ross and Wiglesworth occurring in a Toper; Passage into Secondary Dementia.*—G. B., male, aged sixty, railway policeman; hereditary history unknown; for many years of intemperate habits. Admitted October 1, 1886. A feeble anæmic old man; expression satisfied and not at all unintelligent; he answered questions briskly and without hesitation, but with an absence of orientation that was

most striking. Weeks after admission he had no notion where he was; he seemed to have some vague thought that he was in some institution, for he usually replied promptly to the inquiry where was he—"Meath Hospital," "Limerick Workhouse," "Cork Infirmary," or some such name, generally giving a different one each time. He commonly accounted for the fact that he was lying in bed or sitting at the chimney-corner by saying, "It is Sunday, and you know one can't do anything to day;" but asked where he was the day before he always told a glib story, with all the appearance of conviction—that he had been at some place, perhaps 150 miles away, attending to his work, or the like. The story was quite different at different times in the same day; but it was always ready, coherent *per se*, and improbable only because of the confusion of time and place. He denied that he slept, and usually laughed at the suggestion. "I was away last night on the railway between Sligo and Collooney watching for the rascals who put stones on the line." He nearly always told one that he had spent the previous night at the other side of the island. He was reported to sleep fairly. The waking sense of occupation at night probably corresponded to an active dream state, or may have been merely a reminiscence of former habits of life.

This patient has not recovered. His striking symptoms have become less marked, and he has fallen into a state of general dementia.

CASE V.—*Acute Hallucinatory Confusion resembling Paranoia, associated with Alcoholic Excess; Recovery.*—R. U., male, aged thirty, publican. Admitted March 4, 1889, presenting the ordinary signs of drinking; bloated, greasy, flushed face; full, staring, injected eyes; tongue thickly coated, white, tremulous; pulse bounding; profuse sweating; strong sweetish odour from breath; general tremor and restlessness; no sleep. For the first three days the case was hardly to be distinguished from ordinary *delirium tremens;* afterwards, in addition to confusion similar to that described in the two first cases, there were hallucinations leading to delusions of a somewhat persecutory type, and these did not pass away as quickly as the associated confusion. Thus, he had heard people talking about him lately, saying he had given information to the police, and must be done away with. He told me definitely that he heard this said twice in his back yard, though he did not know by whom, and he concluded there was a conspiracy against him; but to my

colleague, Dr. Nolan, who made some notes of his case, he stated that he saw two men in his yard, and knew what they said, though he did not hear them. Thus, he seems to have had hallucinations both of hearing and vision. He admitted that for some months before the appearance of hallucinations he was nervous and easily startled; the least excitement caused a sense of fluxion to the head, with distressing feeling of confusion. He made an early and good recovery; but has since been again under treatment for a similar attack, in which confusion was less marked, while hallucinations were more prominent.

It must be said that a case like this has a strong resemblance to paranoia; it has not, however, the bad prognostic import of that affection. Before I recognised the form under review, I used to teach that paranoia alcoholica is the only form of paranoia in which recovery may be expected.

Case VI.—*Acute Hallucinatory Confusion simulating Paranoia, following Acute Rheumatism and perhaps associated with Nostalgia.*—D. W. J., male, aged about twenty-five; hereditary taint denied. He was a shop-boy in a remote country village; had always been healthy till November, 1888, when he got rheumatic fever; confined to bed for two months; still very ill and weak for six or seven weeks; as soon as he could travel, came to Dublin for a change (March 15, 1889); felt timid and confused; could not sleep; thought the folks in the hotel looked suspiciously at him, consequently he spent most of the day walking in the street; there he used to suffer from sudden paroxysms of dread (agoraphobia). When admitted to the Asylum, March 28, he could tell little or nothing of what had really happened since he came to Dublin; he did not know the day of the month or week; he was exceedingly confused, and a little incoherent; at first, rather suspicious; he soon became communicative and told a confused story of people coming nightly into his room to murder him, talking about him, pointing at him in the street, and so forth; to escape, he had fled from his hotel the previous night and taken up his quarters in a low lodging-house; here he had hardly got into bed when he heard people at the door sharpening knives and preparing to murder him; in an agony of apprehension he threw himself out of the window, and, in spite of a badly-sprained ankle, ran violently through the streets till he was arrested. This patient was removed from the Asylum by his friends after a few days, so that the sequel is unknown.

CASE VII.—*Hallucinatory Confusion associated with Phthisis.*—B. D., male, aged twenty-one, a clerk, whose sister died of consumption, but whose family history was otherwise good, was admitted to the Richmond Asylum on April 11, 1889, suffering from pulmonary trouble, of three months' duration, which had already produced excavation, and marked hectic. He remained in the Asylum eleven days, and was then removed by his friends to die at home. On admission, he said he had been annoyed for the last six weeks by hearing his thoughts spoken, which, he used to think, was done by someone to annoy him; but he now believed this notion was merely fanciful; by day, save in the early morning, he was lucid, but at night he was a prey to horrible hallucinations and dreads; thought he heard a sawing going on constantly, which was some one performing a terrible operation upon him; thought sometimes he was suffocated with silica; sometimes that he was dead; instead of sleep, he said a terrible mental confusion came over him in which he could not distinguish true from false. Dr. Nolan made this note:—"April 20—Every morning is in a confused state, talking of the horrors of the previous night incredulously, and yet as if apprehending some danger." The analogy of this poor fellow's condition to ordinary feverish delirium need not be pointed out.

CASE VIII.—*Acute Hallucinatory Confusion, dependent perhaps upon Nostalgia; Passage into Dementia.*—M. J., female, single, aged fifty, of peasant class; admitted to Richmond Asylum, March 21, 1889. She had been arrested for wandering in the street. She rented a small farm in the County Armagh, and came to Dublin to see her landlord's lawyer; for no apparent cause she conceived that all the folk in the inn at which she stayed had been boycotted; she knew it by their strange look; she also heard people at the street-corners talking of it, and saying she was boycotted and would have to die; she became so frightened that she wandered away. She was tolerably lucid on the 21st and 22nd, and seemed to be getting over her terrors.

March 23.—When awakened, said she was dying and could not walk; she lay all day in a state resembling hysterical coma or acute dementia, quite motionless and without response; not resisting; keeping the eyes closed; eyelids tremulous.

"March 24.—Saw black men in the room with her last night; fears they will murder her."

After this she became very incoherent and confused; now and

again complained of visions of black men; was occasionally very irritable, and sometimes exhibited maniacal outbursts.

She has not recovered, but has gradually passed into a state of chronic weak-mindedness which does not offer much hope of cure.

This case also appeared somewhat like paranoia at first, but the appearance of fresh hallucinations and unsystematised delusions gives the clue to the true nature of the case.

Case IX.—*Acute Hallucinatory Confusion, beginning in a dream; Apparent cause Sexual Irregularity.*—N. E., male, aged 33, single, bookmaker; hereditary taint denied; said to have been always temperate, but of rather nervous habit.

As a boy he indulged in excessive masturbation; later on suffered from frequent seminal emissions; consequent uneasiness; passed through the hands of a number of quacks; read their literature, and believed himself impotent; tried connection; failed; was more unhappy than ever; mind filled with constant thoughts about, and sensations connected with the genitalia. Examining him, when convalescent, one found that the prepuce was rather tight, and that there was undue sensitiveness about the parts; examination producing evident excitement.

Was in his usual health on May 31, 1889. On that evening he was vexed by his mother asking him some question connected with money, which he thought implied that he was gambling; went to bed at his usual hour and slept till 2 a.m., when he dreamed of the devil, and, awaking, thought he was in the room; ran down stairs and into the street in his shirt; brought back and put to bed; slept; when his mother came to wake him late in the morning he took her for the devil and attacked her. His subsequent recollection failed at this point—fights, devils, policemen, vaguely remembered in a cloudy way.

June 3.—Admitted to Asylum; his friends said he had complained of headache for some days past; he was silent, violently resistive, feverish. (Evening temp. 103°).

June 4.—Lay silent in bed, with fixed staring eyes; body to same degree rigid; rigidity easily overcome; no flexibilitas cerea; would not take food voluntarily, but swallowed what was put in his mouth. (Temp. normal). Spoken to loudly, and shaken up, he said, in a clear, loud, monotonous, absent voice—"I am with the Lord."

June 5.—No rigidity; said nothing, save "I am with the Lord;"

began to take food; left to himself lay motionless, apparently in an ecstatic state; wet and dirty.

June 6.—Dull, exhausted, and dazed; did not know where he was, but made an attempt to discriminate individuals, calling me "sir," for instance.

June 9.—Rapid improvement; was able on this day to give the previous history recounted above, which so far as it could be tested proved perfectly accurate.

June 24.—Discharged recovered; no relapse.

The above cases are selected as examples of common forms of this affection. An interesting case of alcoholic neuritis with mental disturbance, mistaken at first for an aberrant form of general paralysis, would lead us too far afield. So would a description of even one of several cases approaching to katatonia, with their varying symptoms. "Nostalgic" cases generally run a speedy and favourable course. There are often three or four recent examples in my Asylum at the same time.

In conclusion, I have only to say that I do not wish to be understood as setting up acute confusional insanity as a distinct disease. My object has rather been to point out that among the various modes in which acute insanity shows itself there is, in addition to the long-recognised forms of mania, melancholia, and acute dementia, a pretty definite type characterised by confusion with hallucinations. If this type is frequently recognisable, if it is found to occur in connection with definite ætiological moments, if it has anything like a distinct course, if it offers any special indications for treatment, if it can be held to have any particular prognostic significance, then it is distinctly worth while to study its clinical peculiarities and to differentiate it from other forms of mental disturbance, even though one should not esteem it a distinct disease, and although we are quite ignorant of its "pathology" (to give that word the limited sense in which it is commonly used in this place).

ON THE RECENT REVIVAL, UNDER NEW NAMES, OF SOME OLD FALLACIES BEARING ON MEDICINE.

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[Read in the Section of Medicine, May 16, 1890.]

A RECURRENCE of waves of epidemic empiricism widely affecting the practice of medicine has been noticed at periodic intervals in every age of the history of our art. Those therapeutic methods, and the theories connected with them in several instances, have been founded on a substructure of truths that outlived the often unconscious exaggerations and delusions with which they were thus intermixed, and so eventually proved serviceable to the progress of medical science. Still more frequently their prevalence, however short-lived, has been no less injurious to the character of the profession than to the interests of the public. To such methods, and to the undue importance ascribed to them, may, I think, be fairly applied the term—perhaps more expressive than elegant—"Fads," recently borrowed from the vocabulary—rich in terse adverbs—of our transatlantic cousins. But whatever they may be called, whether it be theories, or delusions, or fads, matters, I think, very little, provided the fact be recognised that these epidemic popular beliefs, affecting medical questions, generally die hard, and, after a period of oblivion, are not unfrequently resuscitated even in a more enlightened age. At the present moment, for instance, we have at least "three Richmonds in the field," wherein, as of yore, medical science and its counterfeits are still in close competition in popular estimation—namely, "Hypnotism," "Massage," and "Faith-healing," and each of these may in some measure be regarded as an illustration of the revivalism referred to.

As Lord Bacon has well said—"The river of Lethe runneth as well over as under ground;" and, as we may venture to add, in its course has undoubtedly swept away not a few of the arts and inventions of our ancestors, which have been again reclaimed as new ideas or improvements in our own time. In like manner with the knowledge of our forefathers, so, too, many of their errors and delusions are from time to time revived.

The exemplification of the Psalmist's words—"All novelty is but oblivion," thus afforded by the re-appearance in our own day of the resuscitated theories or the long-buried delusions of our forefathers, is, I think, largely due to that neglect of ancient medical literature which is now unfortunately too commonly characteristic of the followers of what, by courtesy, is still designated a "learned profession." "The mental disease of the present time," says Johnson (and the assertion is surely more applicable to-day than when penned upwards of a century and a half ago), "is impatience of study, contempt of the great masters of ancient wisdom, and a disposition to rely wholly on unassisted genius and natural sagacity. If no use is to be made of the labours of past ages the world must remain always in the infancy of knowledge. The discoveries of every man must terminate in his own advantage, and the studies of every age be employed on questions which the past generation had discussed and determined." Elsewhere I have enlarged on this subject, and have shown that several of our most valued improvements in surgery, obstetrics, and gynæcology—such, for instance, as the employment of anæsthetics before surgical operations, the use of the midwifery forceps, the rapid dilatation of the cervical canal, and the application of endo-uterine therapeutics, are all instances of the revival of ancient and disused practices, as modern discoveries and improvements. For as the father of English poetry well expressed it—

> "—— Out of the olde fieldis, as men saieth,
> Comith all this newe corne fro yere to yere,
> And out of old bokis, in good faieth,
> Comith all this new science that men lere."

Thus the very discussion we are now engaged in was to a large

extent, with regard to hypnotism at least, anticipated as far back as the time of Mesmer, more than a century ago. Of the three commonly supposed novel methods of treatment which have been here referred to, as now more or less epidemic in different countries, the first-named—Hypnotism—is obviously a resuscitation, under a more scientific form and name, of the older practice of animal magnetism or mesmerism. The second—"Massage"—is also but a new expression of an old idea—in plain English (a language apparently somewhat out of fashion in modern medical nomenclature), being neither more nor less than the employment of systematic friction, as a mode of cure, of which the utility was known to Hippocrates, and which was in our own country as successfully resorted to by "the Irish Strokers" of the 17th Century, as by the "Masseurs" of the present day. The third and last of the three "Fads" above alluded to, and that to which this term is most applicable, cannot, however, be considered in this connection. "Faith-healing," as it is called, is the outcome of a religious belief, and hence, however fanatical or erroneous we may deem its disciples, their principles are not matter for discussion in the pages of a medical journal. Neither hypnotism nor massage, however, is entitled to any such special exemption from discussion or criticism, although in some respects it would appear to me somewhat difficult to treat their pretensions seriously. I willingly admit the sincerity and good faith of those who, having convinced themselves of the value of either or both these procedures as therapeutic agents, naturally desire to enforce their views thereon for the general adoption of the profession. At the same time I venture to claim for others, who, like myself, may differ from this conclusion, the right of freely expressing our opinions on questions of such interest as these are to every medical practitioner.

Hypnotism.—Under this term the writers of many of the numerous articles recently published on the subject in the medical as well as in the lay press have apparently confounded together two distinct modifications of what was formerly known as animal magnetism—namely, Mesmerism and Braidism. For both is claimed the poten-

tiality of so influencing, by a non-tangible agency, the mental and physical condition of the person operated on, as to induce a state of hypnotism or artificial somnambulism. In the latter of these two varieties of animal magnetism—namely, that associated with the name of the late Dr. Braid, of Manchester, by whom it was described—this purpose was attained by so concentrating the patient's attention on a mirror, or other similar object, as to eventually exhaust sensorial power and produce a trance-like or unconscious condition, in which, when obtained, as was proved by Braid, Elliotson, and Esdaile, surgical operations may be painlessly performed. Long before Braid's time, however, similar results were secured by the employment of animal magnetism as an anæsthetic. Thus I have found in the *Lancet* for April, 1827, a very interesting account of one of the earliest instances of this kind, in a case reported by Cloquet, wherein he removed a cancerous breast from a patient thus rendered insensible to the pain of the operation. This case was followed by others, and shortly afterwards was investigated by a committee of the French Academy of Medicine, whose elaborate Report was followed by the temporary abandonment of animal magnetism as an anæsthetic agent. The history of its subsequent revival at long intervals by Elliotson and Esdaile, and of its last recrudescence within the past few months, would be beyond the scope of the present communication. There can be no doubt of the possibility of inducing these and other phenomena of Braidism in certain cases, and more especially when dealing with subjects whose powers of volition are weakened by disease, or who are naturally of a hyperæsthetic or distinctly hysterical temperament. The latter class, as I have formerly shown in my memoir on "The Nervous Diseases of Women," is much larger than is commonly recognised, nor is it by any means confined to females, being also widely prevalent amongst men. The expediency or the prudence of attempting to exercise this hypnotic influence on patients, such as those thus most likely to be brought under its dominion, is, however, another question, and one which I should myself unhesitatingly answer in the negative. The objections to hypnotism, apart from the possibility of its abuse, real or pre-

tended, are the uncertainty of its action as compared with other anæsthetic agents, and the permanently injurious effects that would probably be thus induced in the case of a hysterical or hyperæsthetic patient.

There are, however, another class of phenomena that have recently been widely confounded and popularly intermixed with the form of hypnotism or induced somnambulism above referred to, from which they are essentially distinct, and which must, therefore, be here regarded from a wholly different standpoint, though, as I think, with still greater disfavour on every ground, moral and physical. I allude, namely, to those more remarkable alleged powers by which, as has been claimed, the skilled operator in this occult art may at his will and by his mental suggestion or induction of a subtle nerve force, somewhat akin in its supposed action to the magnetic influence, control the thoughts and acts of the hypnotised subject, and even thus modify the course or arrest the progress of disease. These—although, as just said, very commonly confounded with Braidism—are obviously traceable to the older illusions of animal magnetism or of mesmerism, of which they are substantially the resuscitation in a new guise. Animal magnetism, or, in other words, hypnotism, has been defined as a reciprocal influence, supposed to exist between certain individuals, which is brought into action by the will on the one side and by a peculiar susceptibility on the other. This influence, we are told by the initiated, consists in a peculiar subtle fluid or force transmissible, under certain conditions, from one individual to another by the volition of the operator whose suggestion may or may not be expressed by an outward sign or act. As far back as the time of Van Helmont we find the supposed powers of animal magnetism referred to in jargon little different from that employed by some of the hypnotists of the present day—"Magnetism is a universal agent which penetrates all matter and agitates the mass of the universe. We can attach to a body the virtues which we possess, communicate to it certain powers, and use it as the means to operate salutary effects. . . . There exists in man a certain energy which can act beyond his own person, according to his will or his

imagination, and impart virtues and exercise a durable power even in distant objects. Will is the first of powers.[a]

The real marvel connected with such assertions appears to me to be the fact that at the present day some men, of whose sincerity and sanity there is no question, should claim these powers, and that others, similarly circumstanced, should admit the possibility of thus influencing any persons, save those of abnormal, nervous, or mental constitution, and more especially the oftentimes semi-insane victims of hysteria.

A priori, it might well seem incredible that pretensions of this kind should be gravely advanced and accepted in the last decade of the 19th century were it not that this age, too often sceptical of the truths of Divine Revelation, has afforded so many illustrations of its credulity in the illusions of pseudo-scientific enthusiasm, and that at the present time we have abundant contemporaneous evidence of a wide-spread credence in the alleged and incomprehensible powers of animal magnetism as reported under the name of hypnotism. It would be needless here to multiply the proofs of this belief which might be adduced from recent periodical literature. One will suffice for our present purpose—namely, the circumstance that within the past month it has been found necessary in France, by the Minister of Marine, M. Barbey, to issue an official prohibition of the further practice of hypnotism in the Navy under any pretext, by an order addressed to all Admirals now holding command in the French Service. To deny *in toto* the possibility of phenomena, to the actuality of which so many witnesses have testified, merely because they are apparently utterly at variance with common sense and wholly inexplicable in the present state of our knowledge, might perhaps be thought unphilosophical. Hence, whatever our own opinion may be on the subject, we must be content to leave its final decision for the eventual judgment founded on the better knowledge and experience of the profession. Whatever that verdict may be, it cannot be very long delayed. Nevertheless, the countenance recently given by certain medical authorities

[a] Johanes Baptistæ Van Helmont. Opera Omnia, p. 235. Frankfurte, 1707.

to the revival of hypnotism has given a stimulus and apparent support to some popular extensions or perversions of these practices which they never could have intended, and which are much to be deprecated. This is, I think, sufficiently exemplified in a recent article in the *Hospital Gazette*, the able editor of which—to his credit—has been a consistent and outspoken opponent of the attempted resuscitation of hypnotism in medical practice. That criticism may be here briefly cited—"The article on hypnotism, in the present number of the *Universal Review*, is a terrible example of the adumbrating effect of dabbling in pseudo-occultism and mystic lore. Mrs. Besant opens her paper with an allusion to a body calling itself the 'London Hypnotic Society,' which, she says, was founded for the systematic study and use of hypnotism. We were present at the first meeting of this body, and would remind Mrs. Besant that the 'systematic study and use' of that elusive agency, known as odilic force to the initiate—Anglicè, animal magnetism—was also contemplated. We recollect the lecturer on that occasion, in alluding to this mysterious influence, spoke of it as streaming from the eyes, the extremities of the digits, and the toes of the operator, and, if we mistake not, the tip of the olfactory organ was also said to be a medium through which the accomplished magnetiser distributed some of his superfluous nervous energy." The painful exhibitions of so-called hypnotic influence, described in recently published reports of certain proceedings on the Continent, as well as the spectacles of either fanatical enthusiasm, or else of charlatanism acting on acquiescent imbecility, which I have myself on several occasions witnessed in the performances of some professors of animal magnetism, can hardly be spoken of from my point of view save in terms that might perhaps be deemed offensive by those who are believers in these powers. Hence I forbear any further reference to them, for, as the learned Fuller long since observed, "I meddle not with those Bedlam plaucies, all whose conceits are antiques, but leave them for the physician to purge with hellebore."[a]

Massage.—Having referred so fully to the resuscitation of animal magnetism in the preceding observations on hypnotism, space pre-

[a] The Holy War. By Thos. Fuller, B.D., p. 187. London, 1645.

cludes any lengthened notice here of the second of the recent revivals, which, in their epidemic or popular form, may be classed amongst the "fads of the present day." Old facts cannot be metamorphosed into new discoveries by the simple process of re-naming them. The cure of disease by a systematic course of rubbing, or, as it is now called, "Massage," is an idea coeval with Hippocrates, by whom it was recommended.

Celsus informs us that Asclepiades by friction could calm a phrensy; and further states that when these frictions were carried to too great an extent, they brought on a lethargic state. Celsus Aurelius recommended manual frictions for the cure of pleurisy, lethargy, and various other maladies, describing the manner in which they are to be conducted. For instance, in epilepsy, he says that the head and forehead are to be chafed, then the hand is to be carried gently over the neck and bosom; at other times the extremities of the hands and feet are to be grasped, that "we may cure by the very act of holding the limb." Hence the popular idea apparently fostered by some persons that massage is a new procedure must be included in that category of ill-appreciated teachings of which we have a typical example in the case of the ingenious youth who desired to impart to his aged grand maternal relative the occult art of extracting the ovuline constituents from their shell.

Within the past two or three years, however, the attempts to introduce this supposed new method into general favour have been most successful, at least if the number of its practitioners affords any criterion of the popular demand for their service. We now find massage developed into a distinct specialism, with a name as high-sounding, pretensions as extensive, and followers as numerous, as some of the other specialisms of modern medical practice. This is sufficiently shown by the advertising columns of the daily press, medical and lay, in which may be read the announcements of the countless array of professors of massage, male and female, who thus prefer their "certificated" readiness to rub away, for a small consideration, many, if not all, "the ills that flesh is heir to." I shall not presume to discuss the value of this procedure, which we all know to be useful in certain cases. For aught I can here say, the

"masseur or masseuse" may be able or be powerless to accomplish all they or their supporters claim. What I, however, would desire now to call attention to is the circumstance that a method of treatment largely anticipatory of the present practice both of massage and hypnotism was practised in this country so far back as the days of the Commonwealth, and afterwards during the reign of Charles the Second, with a temporary success, probably not since surpassed. The long-forgotten story of this strange episode in the history of popular or pseudo-medical phantasmata was disinterred nearly half a century ago by my father, the late Dr. R. R. Madden, by whom it was very fully narrated in the second series of the old *Dublin Quarterly Journal of Medical Science.*[a] As that history is now generally unknown, and as the journal itself is long since out of print and inaccessible, I may in this connection here briefly recapitulate the circumstances which he there referred to.

The first person in this country who performed cures by stroking patients with the hands is commonly supposed to have been Mr. Valentine Greatrakes, who flourished in the reign of Charles the Second. That supposition, however, is erroneous. A much more remarkable performer of this kind figured in Ireland in the troubled times of the Commonwealth, as well as after the Restoration. An account of his "wonder-working practices" exists, drawn up with great care by a contemporary having a personal knowledge of the man, his pretensions and performances.

In the following notice, says Dr. R. R. Madden, some interesting particulars and remarkable statements of facts, in relation to practices analogous to those of modern animal magnetism, will be laid before the reader; and it is for him to make his own deductions from them. Those who set about building up systems, before they have collected sufficient materials for the projected structure, begin at the wrong end of their undertaking, and perhaps the disposition to demolish everything newly raised, from a fear of novelty that exaggerates defects, may lead to the untimely end of beginnings that might possibly have grown into some-

[a] The Mesmerists of Ireland of the Seventeenth Century. By Richard Robert Madden, M.D., M.R.I.A. No. VI. 1847.

thing good or useful, had the work proceeded. In things that are strange and new, every failure is accounted by some a proof of imposture, and in others a great number of failures do not excite any suspicion of the power or probity of the performer. Before entering on the subject of the first wonder-working, non-professional practiser of the healing art I have referred to, the principal circumstances that are known of Greatrakes require to be briefly noticed. He was born in 1628, at Affane, in the County of Waterford; educated in the free school of Lismore; and "perfected his studies in Humanity and Divinity in England," in the house of his maternal uncle. The breaking out of the rebellion had caused the flight of his family, and the embarrassments of the latter led to his return to his native land. Afflicted at the deplorable state of the country, and disgusted with the unnatural differences then existing, he retired from the scene of strife, and sought seclusion in the Castle of Cappoquin. "There," he says, "I spent a year's time in contemplation, and saw so much of the madness of the world, that my life became a burden to me, and my soul was as weary of this habitation of clay as ever a galley-slave was weary of his oar." In 1649 he obtained a commission in Lord Broghill's regiment, then on service in Munster, and in 1656, when a part of the army was disbanded, he retired to his estate at Affane, and shortly after was appointed a Justice of the Peace. About 1662, when he was thirty-four years of age, he says he had "an impulse, or a strange persuasion in his mind (of which he was unable to give any rational account to another), which did frequently suggest to him that there was bestowed on him the gift of curing the King's Evil." This conviction he concealed for some time, but at length communicated it to his wife, who endeavoured to persuade him it was some strange imagination; whereupon he made his first experiment in her presence on a youth afflicted with the evil, "grievously in the eyes, cheek, and throat." "Then I laid my hands on the places affected, and prayed to God to heal him, and bid the parent bring the child two or three days afterwards to me again, which accordingly he did, and I saw the eye was almost quite whole and the throat strangely amended, and, to be brief, within a month

discharged itself, and was perfectly healed, and so continues." He now proceeded on his career of healing by means of prayer and friction, or stroking the affected parts, and his fame extended over the country. In 1665, at the request of the Earl of Orrery, Greatrakes went to England "to try his hand on the head of Lady Conway of Ragley, in Warwickshire," who had long laboured under some cerebral affection.

He remained at Ragley some weeks, and totally failed in his efforts to cure or relieve that lady, but "several marvellous cures performed in Warwickshire by the stroking of the hands of Mr. Valentine Greatrakes" are related by Mr. Stubbs, a medical practitioner of Stratford-upon-Avon, in a letter containing an attestation of several other cures by "the miraculous Conformist," as the poor Irish gentleman was called, given by Mr. Foxcraft, M.A., Fellow of King's College, Cambridge. In Stubbs' letter he speaks thus of the performances he witnessed, "filling with admiration the most learned and suspicious beholders. He is a man of a graceful personage and presence, and if my phantasy betrayed not my judgment I observed in his eyes and mien a vivacity and sprightliness that is nothing common." The writer proceeds to describe him further as a man of good life and of benevolent principles "seeming to include some grains of the golden age, and to be a relic of those times when piety and miracles were sincere." In his attempts, however, to give a natural solution of most of the cures effected by Greatrakes, he makes mention of his "*long and continued frictions, from one to two and three hours;*" and Dr. Smith, in his "History of Waterford," reminds his readers of Bacon's observation, in his "History of Life and Death," that motion and warmth (of which true friction consists) draw forth into the parts new vigour and conduce much to longevity. "How far the imagination, in those alleged cures, he adds, might contribute to the cure is uncertain."

The fame of Greatrakes reached the Court of Charles II., "who invited him to Whitehall, to exhibit his powers." The Earl of Arlington was the bearer of the King's commands. The merry monarch might have been somewhat jealous of the renown of a

successful rival in the practice of manipulating patients for the cure of scrofula. One of his Majesty's first public acts after the Restoration Evelyn thus describes, July 6th, 1660:—According to custom, His Majesty sitting in the banqueting house, the chirurgeons cause the sick to be brought or led up to the throne, where, they kneeling, the King strokes their faces or cheeks with both hands at once, at which instant a chaplain in his formalities says, "He put his hands upon them and he healed them."

This Evelyn details in his usual quiet manner, and having described the patients getting a piece of money strung on white ribbon put round their necks by his Majesty, he adds, "The chaplain, each time the King put on one of these ribands, repeated, 'That is the true light who came into the world.' The gospel was then read, and prayers for the sick offered up; a blessing pronounced, and the King washed his hands after having stroked above six hundred." In the memoirs of Lord Orrery in manuscript, Anno 1669, cited by Harris, Mr. Love states that Greatrakes failed to relieve him of a pectoral and rheumatic disease, but he was witness of his curing the falling sickness, and further that the Royal Society, and other modern philosophers not able to dispute the fact, found words to define it, and called those strange effects a sanative contagion in the body, which had an antipathy to some particular diseases, and not to others. In the Philosophical Transactions, No. 256, p. 332, the celebrated Ralph Thoresby gives several remarkable instances of cures effected by Greatrakes. He mentions the practice adopted by him in the case of his own brother suffering from pain in head and back, who obtained immediate ease to his head by the former only stroking it with his hands; he then fell to rub his back, which he most complained of, but the pain flew from his hand to the thigh, thence he pursued it to his knee, from thence to his leg, ankle, and foot, and at last to his great toe. As it fell lower it grew more violent; and when in his toe it made him roar out; but upon rubbing it there it vanished.

Some of his failures in England caused a clergyman of the name of Lloyd to write a book against him, entitled "Wonders no Miracles, or Mr. Valentine Greatrakes' gift of healing examined."

(London. 4to. 1666). The latter vindicated himself in a publication named "A brief account of Greatrakes, and divers strange cures by him performed, in a letter to the Hon. Robert Boyle, Esq." (London. 4to. 1666). Lloyd's severe reflections on the reputation of the "Miraculous Conformist" seem to have borne down his reputation in England; and on his return to his own country nothing foreign was requisite to bear out in his case the truth of the old adage that "No man is a prophet in his own country."

The last time that any mention is made of him by his contemporaries was in 1681. He was then living in Dublin, but how long he lived after, says Dr. Smith, is uncertain. In 1673, when Peter Walsh, the Franciscan, produced his "History and Vindication of the Irish Remonstrance," Greatrakes appears to have sunk into obscurity. Walsh says "that no sooner had a Catholic wonder-worker made his exit than Greatrakes, an English lay Protestant, started up to supply the former's place, by making people believe he himself, too, had a gift from God to cure all diseases by praying and stroking; even also at London, whither he came at last to cheat the world, as the former was thought to have done. What became of this Greatrakes I neither know nor care; only this, I know that not long after his practices on folks in London he went out like the snuff of a candle, just as Finaghty did." Father Walsh, when Procurator of the Franciscans, was called on, in 1666, to lay before the National Congregation assembled in Dublin a detailed account of all he had either heard from others or by his own experience known of the far-famed wonder-working priest, Father James Finaghty. It is to be borne in mind that Father Walsh had taken a prominent part in the affairs of the Catholic Confederation against the Cardinal Rinuccini and in favour of the Viceroy Ormond, and, by his own admission, Father Finaghty had been of the opposite "Nuncio party" in the late troubles of the nation. Ormond again, in 1666, filling the same office, patronised Walsh, who was on very intimate terms with him. After the departure of the latter from Ireland, so little were the tendencies of Father Walsh towards enthusiasm in religious matters, that in England he obtained some appointment. The first time and place

that he heard of the remarkable personage was in London in 1657, during the reign of Cromwell.

In a letter from an Irish Jesuit to one of his Order in London there was an account of one "James Finaghty, a secular priest, who formerly had charge of a parish in the Archbishopric of Tuam, apparently raised up by Providence in those times of trouble and calamity to confirm the people in their religion, and for this purpose gifted with a true miraculous power of dispossessing devils and curing all sorts of ills and diseases, so that he drew the world after him, and not only Catholics but Protestants, insomuch that he had often a thousand, sometimes fifteen hundred, nay, two or three thousand, who followed him even through bogs, woods, mountains and rocks, and desert places, whithersoever the people heard him to have fled from the persecution of Cromwell's officers and governors, that priests enough could not be had (though many accompanied him on purpose) to hear the confessions of the great multitude drawn to repentance and resolutions of a new life by the example of his life and wonder of his works, that, therefore, he was esteemed a Thaumaturgus, or wonder-worker of Ireland." Readers who may feel disgusted at accounts of superstitious practices or occurrences in Ireland, and disposed to relinquish all research when these are to be met with in their own country, even in the chronicles of events two centuries ago, may be reminded of the doings of Mr. Thom, "The New Messiah," and his followers in the vicinity of the principal cathedral town of England within the present age, and may be assured that a great deal more interest attaches to the performances of the Irish Thaumaturgus than to the phrensied exploits, impious pretensions, and the tragic catastrophe of the great Canterbury prophet, or even to the more recent performances by rival practitioners in the same line that have been witnessed in St. James' Hall, London, or at the Salle des Conferences, Paris, during the past few weeks. There are matters of interest, even in a medical point of view, in the singular effects produced on the imagination, and, as it is alleged, on disease itself, in many instances, by the practices of Finaghty, and, notwithstanding his frequent failures, it is a question of more than mere curiosity whether his practices

of a curative kind were identical with those which Mesmer is commonly supposed to have been the originator of.

It is certain that practices similar to those of the animal magnetisers were in vogue with the Pagan Irish, and were condemned by Saint Patrick, but, notwithstanding the condemnation, were still observed after Christianity was established. One of the first Irish scholars of this century, the late Mr. Eugene Curry, recognised in a Pagan practice, called in Irish *Team Lac*, or the "Enlightenment," a performance of a mesmeric kind. The earliest account of this practice was found by that gentleman in an ancient Irish manuscript, where the affairs of Ireland are treated of during the interregnum between the death of Edirsgel More and Conair More, about the year of our Lord 20. "The evils arising from the delay attending the election of a king after the death of Edirsgel led to a great assembly of the princes and chiefs at Tara, and there it was determined to practise the *Team Lac*, to ascertain the will of heaven respecting the succession. The 'Enlightenment' was accordingly performed in the following manner:—A young man was selected by the Druids—a great number of solemn rites were performed; one of these consisted of breathing on him; and these operations continued till he was put into a deep trance, and while thus entranced he was questioned as to the person who was destined to succeed Edirsgel. He then returned intelligible answers, describing the person spoken of. The young man was kept entranced till the Druids sent to a place six or seven miles distant, which had been named by the young man, and there they found the person described and sought for, and they forthwith proclaimed him monarch."

In 1662, the Duke of Ormond being again Viceroy, Walsh, on his return to Ireland, heard once more of Father Finaghty working great wonders. He was commissioned, immediately after his arrival in Dublin, by his Grace of Ormond, "to look particularly after Father Finaghty, and see he allured the people no longer, by going about like a mountebank cheating all the nation." Walsh then was appointed not to inquire into the truth or falsehood of Finaghty's pretended powers, but to "see that he allured the people no longer"—so that we are more likely to find in his report what was

unfavourable to the pretensions of this man, in the result of his performances, than such things as might seem to give any support to them.

The result of his inquiries was, that in 1662, previous to Ormond's arrival in Ireland, Finaghty had obtained a pass from the authorities "to go freely where he pleased in Ireland," and had gone from province to province, and had drawn after him great multitudes of people, "same expecting to be healed of their infirmities, others—the greater number—to be satisfied in their curiosity;" that he had blessed a great number of wells in various places, and had given them special titles, and, as the people imagined, had given them part of his own supernatural virtue for the cure of diseases; that he had made a triumphant progress through several counties till he came within five miles of Dublin, and was in all places received, reverenced, and entertained, not only by the common people, but by the gentry, nobility, knights, lords, ladies, and clergy also, and by many presented with gifts; that at Cluansillagh, five miles from Dublin, such vast numbers of people of the city and adjacent country thronged about him, that many were almost trodden to death; that from thence he returned to his own country, practising everywhere, as his manner was to practise in the fields amongst great multitudes, dispossessing of devils, from such as he was pleased to judge possessed, and curing too, or at least pretending to cure, all other diseases by prayers, exorcising, stroking, blowing vehemently into the ears of the diseased party; that some Protestants had gone to see him, others of them to get his helping hand, but amongst them many who returned extremely dissatisfied, looking on his feats as impostures, whilst others cried them up as miraculous performances, and from the former dissatisfied persons the information given to the Lord Lieutenant had proceeded." A Galway man, well acquainted with Finaghty, told Walsh that the former had in his youth been a servant to one Father Moore, an old venerable Jesuit, and skilful exorcist, from whom he had learned his art and knowledge of exorcising. Subsequently Finaghty, when questioned on this point, denied the assertion. There was no Jesuit on the Irish mission named Moore. There was one named Moore living in Drogheda in

1642 and 1649; he was superior of his brethren in Galway, then a septuagenarian, a man of great repute in his order.

Walsh could meet with only "two grave Catholic gentlemen—Geoffrey Browne and Richard Bellings of the Kilkenny Confederation—who could speak from their own knowledge of cures effected by Finaghty." These gentlemen were "no bigots." Bellings assured him that Finaghty had "a wonderful gift from God of curing by exorcisms and prayers." Browne told him he had been present when a cripple, well known on the way, who had always been without the power of his lower limbs, going upon all fours, with the aid of two short crutches half a foot high, had been cured in the presence of a great multitude in the following manner:—The cripple being laid down at the feet of Father Finaghty the latter prayed over him, and after some time bid him get up, which the cripple did not, but one of the assistants raised him with difficulty, the cripple crying out vehemently that he was in torture all the time; then he was laid down again, and Finaghty repeated all the former operations, and bid the assistant raise him up once more; the cripple was no sooner raised up than he stood without assistance, walked before all present, gave God thanks, and found himself perfectly cured; and the following Sunday attended a hurling match, "to the extreme wonderment of all who had known him before." Mr. Browne, however, "would not determine whether what he saw was a true miracle or not." About a year later Walsh received accounts of Father Finaghty, then in England, performing wonderful cures both on Catholics and Protestants at the Portuguese Ambassador's in London, and at the Queen's at St. James's.

It appears that he was sent for to London to cure the lady of a Portuguese nobleman, then staying with the Queen, who had lost her sight, and Father Taig M'Eochny, *alias* Captain Power, an Irish Dominican, afterwards Bishop of Clonfert, was sent from the Court for Father Finaghty. The mission was in vain; the attempts of Finaghty to restore sight to the blind countess proved a failure. Nevertheless, he maintained his credit with his friends, though the jealousy of some minister caused him to decamp from London.

He was thence honourably conveyed in a coach drawn by six horses, accompanied by an English Catholic gentleman, through Oxford to Chester and Holyhead. He remained in Wales for some days with Lord Dillon and Mr. Garrett Moore, two great admirers of his, and then returned to Dublin. At Castletown, in the Diocese of Dublin, at Lady Dangan's, Walsh was informed that he had healed a lame man, and given sight to a blind one. But the parish priest of Castletown seemed not thoroughly satisfied of the miracle done here. While at Lady White's, at Leixlip, he restored the sight of a blind man. Yet neither could be said to be perfect cures—"the blind man did not see clearly, nor did the cripple go even then strongly and confidently." Walsh at last met Father Finaghty in Dublin, but previously he found that some Protestant officials "had come to a resolution of bringing Finaghty before the Bishop's Court, for a wizard or an impostor," and that Dr. Loftus interposed, and objected to that proceeding till the Viceroy's return to Dublin. The Roman Catholic clergy had assembled, and discussed the propriety of prohibiting any further practices of his in Dublin. The Franciscans alone continued to countenance him in any way, and a Protestant Irish gentlewoman, they informed Walsh, had been cured of some disease in her limbs. The lady herself also informed him of the same fact.

This was the only cure which Walsh could learn had been effected by him, and he attaches little importance to it. He then gave the result of his own communication with Finaghty, whom he first met at the chapel of Father Aylmer, a secular priest, in Owen's Arch (Audeon's Arch), when the latter was preparing for the Altar, and was on his knees when Walsh entered. After visiting him several times he had at length an opportunity of seeing him practise on twenty or thirty poor country people. Their ailments were pains in various parts, weakness of sight, and deafness. One was a boy apparently blind, and another a girl of thirteen or fourteen, who "was troubled with the fairies." He began with prayer and exorcism; these were very short. Having driven the pain, as the patient asserted, from one part to another, he followed it from place to place, praying, until the patient being

questioned answered, "cured." Then he bade the patient return thanks to God kneeling. In this way he proceeded from one to another, but occasionally varying the practice. In some cases he used to blow very long and very strong into the ears of such as complained of deafness or pain in the ear, laying his mouth on the affected ear, blowing so vehemently that, without any miracle at all, it was effectual to work in the organ some alteration. The practice of blowing into the ears of cattle, especially horses, is an old popular arcanum well known to cattle doctors and veterinary practitioners in this country. Finaghty's repeated efforts to restore the sight of the blind boy were unsuccessful. A woman from Crumlin with the child troubled with the fairies next approached, and after praying and exorcising he said that she must be brought to him again.

A few days afterwards, two distinguished gentlemen of the Court, Sir William Petty, the famous physician, and one of the clerks of Council at Whitehall, Sir Robert Southwell, who had lately been Envoy Extraordinary at the Court of Portugal, came, unexpectedly, as Walsh states, to examine the sick persons, who were then waiting in town for the trial. Sir William Petty spoke to Father Finaghty in courteous terms, saying, "You may write in my soul what you please as to the way of worshipping God, if you attest that way by plain miracle; remove this wart which you see on my finger and I will declare myself of your religion." Finaghty objected to the proposal or to any deductions that might be drawn from its failure if accepted, so it was withdrawn. Sir William then said he laboured under a troublesome infirmity, being near-sighted, and if Father Finaghty would cure him he would humbly and gratefully acknowledge "God's merciful and wonderful hand therein." Father Finaghty rose up and said, "Let us try," then knelt down to pray. He then placed Sir William between him and the light of the window, and said a prayer proper for the eyes. Then he put a Bible in his hand, and asked him could he read it better than formerly. The latter at first thought his sight "mightily amended," but soon finding his error, he told Father Finaghty how it was. The operation was repeated with the same

result a second time. Sir William asked Walsh if he knew anything of necromancy, for he knew a book that treated of that science, wherein the prayer used by Finaghty was to be found. The book was then shown to Walsh, for, it strangely appears, Sir William had it in his pocket, and before his departure said he had a proposal to make—let Finaghty assemble in a field a multitude of sick people, as he is wont to do, and he (Sir William) would bind himself in a sum of £100, to be paid in gold to Father Finaghty, to cure by his own practice as many as Finaghty should cure by his. Then he and Sir Robert Southwell went away without so much as saluting or noticing in any way Father Finaghty.

Next day Walsh, by the Viceroy's orders, intimated to him that all was in readiness for the proposed trial. It was evident he never expected the permission of the State would be given for such a trial. He immediately said he could not stay; he must set out for Connaught, as his health had been impaired in Dublin by the labour of mind and body attending his performances; he must go to the country to recruit his health, then he would return and perform all he promised. A sick man setting out on a long journey on horseback in the depth of winter seemed somewhat strange to Walsh, who called upon him to write to the Viceroy his reason for abandoning the proposed trial. Finaghty said he was a poor master of the pen, and could not presume to address a Lord Lieutenant. The menaced prosecution in the Bishop's Court was only suspended, and witchcraft was a felony punishable at the stake. Walsh gave him some money for his journey; charged him to make no further attempts at healing or exorcising. Here ends Father Walsh's report to the Lords and Fathers assembled.

Whether Finaghty, the far-famed wonder-worker, was a mere impostor, a mere fanatic, a compound of both, or a man who performed some cures, which neither a mere fanatic nor a mere impostor could have effected, and whether those cures were made alone by the influence exercised over the imagination, or by practices productive of phenomena (of whatever nature they may be) similar to those produced by mesmeric operations, the reader will determine.

It may be observed, adds Dr. R. R. Madden, that practices analogous to those of animal magnetisers for the cure of diseases may be traced in many works of antiquity, and the popular modes of cure by the medicine men amongst some tribes of American Indians, the halrims in the East, the fetish people in Africa, and the handling doctors in some parts of Ireland. The simple practice of handling, or stroking with the hand, in all probability preceded the use of the magnet for the cure of disease. Lessing erroneously ascribed to Paracelsus the use of the magnet as a remedial operation. Elins, who flourished A.D. 500, making mention of it, says:—"We are assured those who have the gout in their hands or feet, or convulsions, find relief when they hold a magnet. Paracelsus, who surpassed all the wonder-workers of antiquity and believers in marvellous scientific practices, even in the possibility of making 'homunculi' by them, particularly eulogised the use of the magnet for the cure of disease; fluxes and hæmorrhages, and various other maladies, he tells us, were curable by magnetic influence. In the fifteenth century Marcellus speaks of the use of the magnet for the cure of tooth-ache; and Leonard Camellus, for the same disorder, speaks of its application to the head. Kircher mentions the use of magnetic beads worn round the neck; and Ambrose Paré speaks of the employment of the magnet in some surgical operations. In 1770 Father Hehl, a Jesuit Professor of Astronomy in Vienna, Beckman states, communicated to Anton Mesmer the wonderful effects produced by magnetised steel plates.

In 1798 Perkin's magnetic steel tractors obtained a temporary notoriety, but the *modus operandi* both with Mesmer and Perkins, as far as regards the mere manipulation, imposition of hands, passes or friction, was the old popular practice that has been already referred to—the handling or moving of the hands over the sick—of those operators, in various countries, who professed to cure disease by the combined power of magic and medicine, or superhuman means alone, with the influence, real or supposed, of magnetism superadded to it. Whether such phenomena in ancient or modern times have any foundation in actuality it would be difficult as yet to pronounce. "There are more things between

heaven and earth than are dreamt of in our philosophy," and these may be of them.

At any rate, in conclusion of this long article, it may be interesting to bear in mind the success, in one respect at least, that rewarded the original professor of animal magnetism, whose career might possibly be some encouragement to the modern practitioners of hypnotism, and to remind them that upwards of a century ago very similar pretensions were made by Mesmer, whose thesis "On the Influence of the Planets on Human Bodies" was published in Vienna in 1776. Whether Mesmer appropriated the views previously held on this subject by the Viennese astronomer Hehl, as the latter claimed, or not, now matters little. The controversy between the learned professor of unsavoury name and the reputed father of animal magnetism was a very animated one, and contains some curious matter, to which, I trust, I may possibly be permitted to return in a future communication on the early history of this subject. The result of the discussion was Mesmer's retirement from Vienna to Paris, where, two years later, in 1778, he reappeared as the then reigning lion of Parisian society, as well as the most successful practitioner of his occult art, from both of which positions his fall was as signal and as rapid as his rise thereto had been. Whilst the brief sunshine of his prosperity lasted, however, Mesmer, who apparently had always a shrewd eye to the main chance, acted on the old adage so successfully, that, within a couple of years, he realised in Paris a fortune of some 340,000 livres, having, before the adverse report of the Royal Commission appointed to investigate this question, managed to sell the secret of his method for a sum equivalent to £14,000 to the *Societé de Harmonie.* The modern professor of animal magnetism, or hynotism, might well regret the palmy days of the *vieille cour.*

SECTION OF SURGERY.

THE OPERATIVE TREATMENT OF TUBERCULAR DISEASE OF THE KNEE-JOINT.

By WILLIAM THOMSON, F.R.C.S.;
Surgeon to the Richmond Hospital, Dublin; General Secretary to the Royal Academy of Medicine in Ireland.

[Read in the Section of Surgery, November 8, 1889.]

The more general methods of dealing with tuberculosis of the knee-joint, whether of the synovial membrane or of the osseous parts, are not satisfactory. Everyone here knows from experience how he has tried remedy after remedy short of operation, and the weary waiting that he has had for a cure that seldom comes. I do not wish to assert that cases of cure do not occur; but I have come to the conclusion that tuberculous synovitis is next to an irrecoverable condition, at least among the poor, with whom hospital surgeons have most to do. The surroundings of their homes, the necessities of their mode of life, the impossibility of carrying out the ordinary treatment of rest, good air, nutritious food, &c., supply us with a number of cases which we have come to look upon as hopeless in the sense of saving the joint, or even of saving the limb with a stiff knee. That statement I make not only as the experience of myself, but I believe I may say of my colleagues also.

The knowledge that tuberculosis is a distinctly infective process; that its tendency is not only to invade neighbouring structures, but to produce distant deposits, makes it necessary that our treatment should be of a character to remove the source from which the mischief spreads. In the case of the young we are dealing

with a class of patients in whom the pathological processes partake of the activity which is going on in all the tissues. There is, therefore, the greater need of decision at a period early enough to give hope of eradicating the disease. Whether the starting-point be in the bone or in the synovial membrane, the process of invasion is equally certain. The disease spreads from bone to synovial membrane, or from synovial membrane to bone. Not only so, but the extra-articular structures are also attacked, and not infrequently become involved in the general destruction.

Having, then, for our object the thorough removal of all reachable diseased parts, and the saving of the limb, I do not hesitate to say that excision of the joint is the operation which gives the best prospect of success. The remedy may appear to be a severe one, but, as in all other cases, the operation has in these days become comparatively safe.

It may not be uninteresting to refer to the first knee excision which was performed in this city. A writer in the *Lancet*, Vol. VI., 1825, p. 29, describes the operation as done by Crampton:—

"We happened to be present some time back at one of those scenes of scientific butchery at the Meath Hospital. The patient was a female; the complaint, if we recollect rightly, open scrofula of the knee-joint. A great concourse assembled to witness the operation; it was quite a gala day with the dissectors—a festival, seemingly, held in honour of the virtues of *steel*. It was the first time, we believe, that the removal of the knee-joint was attempted here. We earnestly hope it will be the last. The operator, of course, accomplished his purpose with his usual dexterity. But could he have beheld, as we did, the contorted countenances of the spectators, the knife would have fallen from his hand, never to be used where it was not more imperiously indicated. To be present was to be indeed a torture. One man vented his feelings in a wink; a second in a hem; a third overcame his sympathies in a forced fit of laughter; . . . all, to be sure, admired, but all disapproved. . . . We saw this poor creature a long time after endeavouring to drag her limb with her by means of sundry wooden contrivances. . . . Let it be called by whatever name the san-

guinary desperadoes of the profession choose, but we shall never be cheated out of our judgment, or fear to expose such practice when it falls under our observation."

Even thirty-one years (1856) afterwards we find Mr. Syme inveighing against the "vehement and persevering efforts which have lately been made to force into fashion two operations which are bloody and formidable," one being excision of the knee, and the other excision of the hip-joint.

All this is but a further illustration of how surgery has progressed since then. Fergusson in London, and Butcher in Dublin, by their successes, placed the operation upon sure ground, and we no longer look upon it either as bloody or terribly fatal. My own experience extends to seventeen cases, in which I have removed the joint for various causes in patients ranging from 4½ to 39 years, and in no case has a life been lost. In two, which were done some years ago, I had subsequently to amputate, but I have not seen this necessity arise in the last thirty cases in our hospital, since we adopted the methods of fixation which we now employ.

Mr. Thornley Stoker has already brought before the Academy the plan of fixing the divided bones by silver dowels, and he now uses them in all cases; their advantages are very great, and I attribute much of our later rapid recoveries to them. It is unnecessary to describe them here; but I may be permitted to refer to the simple splint which I have now used for nine years in these cases.

In my earlier cases I employed Dr. Heron Watson's method; and in the *Brit. Med. Journal* of January 28th, 1882, I gave the reasons which induced me to adopt a simple modification of his splint. I found that a slip of strong hoop iron in front, with a loop over the wound, and a similar slip behind, allowed the plaster bandage to fit closely to the limb, and prevented painful movement of the bones. It was my habit to fix the splint with ordinary bandages for the first twenty-four hours, so as to permit the opening up of the wound in case of hæmorrhage, and then at the first dressing to apply the plaster-of-Paris. A little experience showed me that the limb rested easily, and was firmly held by the ordinary bandage

which, including the hoop-iron splint, could be closely applied to the limb. I then determined to try a case without any plaster whatever, and for some years I have not used plaster-of-Paris at all. My colleagues observe the same practice, and it has been uniformly satisfactory.

The splint I now use is made of strong hoop iron about an inch and a half wide. It is easily bent into any shape, and it is usually prepared by my resident pupil. The posterior splint is plain, and extends from the gluteal fold to the foot. At the tendo-Achilles it must be bent downwards, so as to avoid pressure, and the continuation is brought up along the sole of the foot. It gives greater support, and enables us to bandage and fit the foot to it. The anterior splint extends from the groin over the thigh, is then shaped into a roomy loop, so as to give space for dressing, and is continued downwards towards the foot, being finally shaped to lie easily upon the instep. These splints are carefully padded beforehand, and the wadding is fixed in position by a spiral bandage. The wound dressing should be applied in front, and then be carried down on either side as far as necessary. The splints are then adapted, and are fixed by an ordinary bandage, and if this is properly employed it will not be necessary to readjust it for some weeks. The last thing done is to pack the popliteal space on either side with iodoform wool, taking care, of course, that the drainage tube delivers all fluids into antiseptic dressing. When the patient has been removed to bed the leg is swung from a strong cradle. Two turns of an ordinary bandage are passed through the loop and fixed to a bar in the cradle above, and another bandage is passed under the splint just under the ankle, and also fixed to a bar in the cradle. This permits the patient to make some movement if necessary, and, as I have already said, he is usually able to sit up in bed on the third or fourth day with a bed-rest, suffering no discomfort.

Several other plans of dealing with tuberculosis of the knee-joint by operation have been advocated. Injections and free destruction of the membrane by the actual cautery have each had their followers. The latest recommendation is that we should

practice erasion of the diseased parts, and gouging if necessary. The arguments in favour of this treatment are many; some of them are excellent in theory; but the results, thus far, are exceedingly unsatisfactory.

The principal advantages claimed for this operation instead of excision are these: first, that the movements of the joint may be preserved; secondly, that there is no subsequent shortening.

With regard to the first claim, I have to observe that all surgeons would be glad if they could secure movement in the joint. But movement is a very relative term, and we should like to have it defined strictly. There may be motion that is rather a disadvantage than otherwise, or we may have motion that is next door to fixity. Just let us remember what is done in these cases of so-called erasion. The whole of the synovial membrane is to be removed, diseased bone is to be gouged away, loose cartilages are to be clipped off, and, in a word, all diseased tissues are to be eradicated. Of course the operation may not be so extensive as this; but still I ask, if all that I have mentioned is done, may we reasonably expect a movable joint, and if so, what is the use of the beautiful arrangements that exist in our knee when they may be all removed without practically interfering with its function?

But the cases we read of show that the claim is after all based on very insufficient grounds, and that in practice it is not a desirable end to aim at. It has been found that there is a very great tendency to flexion of the limb, even so far off as a year after operation. We know that we have to guard against this even in cases of excision in young children. And, therefore, we are not surprised to learn that as the result of experience this claim of movement is being gradually abandoned, and ankylosis is being sought for.

But why should it be attained in this roundabout fashion? Can it be secured as quickly between the articular surfaces of the bones, kept in fixed position, as between two level freshly-divided surfaces? Certainly not. It is, of course, said on the other side that we ought not to remove parts which are not diseased. That has only a certain element of truth about it, which may mislead us. I have seen cases in which, with cartilages looking perfectly healthy, we

have found, on section of the bones, isolated necroses or nests of tuberculous matter. Now these are just the cases which the thorough-going eraser would regard as typically good for his method; yet, if the sections had not been made, we should have shut up the disease we wished to get rid of.

The second claim—that shortening does not occur—does not demand much examination. Those who urge it as a late danger in excision seem to forget that it sometimes occurs in cases where no operation of any kind has been attempted; and, therefore, it cannot be necessarily the result of excision. Of course, the amount to be removed in our bone section simply depends upon the extent of the disease. We try to avoid any encroachment upon the epiphysary line, but we must encroach if the disease brings us there. Our object is to save the patient's limb; and if we have to do that at the expense of much shortening, we cannot help it. But my own experience has not been of this kind; and I believe it will be found that this misfortune is ideal in the great majority of cases.

I believe that once we determine upon cutting open a knee-joint in general tuberculosis of the synovial membrane, we had better go the whole way, and perform the ordinary excision. The operation is, I maintain, safer and more satisfactory in its general results than erasion. My own hospital experience, and as far as it extends to the practice of my colleagues, is a mortality of one in over fifty successive cases. That leaves little to be desired; and I am satisfied that the patients who have been submitted to excision of the joint have on the whole been better treated than if they had undergone exploratory incisions and tentative scrapings at the hands of a surgeon.

COMPOUND LUXATION OF THE ANKLE-JOINT. ILLUSTRATED BY CASES WITH SPECIAL REFERENCE TO THE PRESERVATIVE SURGERY OF THE FOOT.

By HENRY GRAY CROLY, F.R.C.S.;
Senior Surgeon to the City of Dublin Hospital; Vice-President of the Royal College of Surgeons, &c.

[Read in the Section of Surgery, December 6th, 1889.]

THREE cases of compound luxation of the ankle-joint—one of the tibia and fibula forwards, one of the tibia inwards, and a third of both bones outwards—having occurred in my hospital and private practice (two of the cases quite recently), I wish to place them on record by communicating them to the Royal Academy of Medicine, chiefly with reference to the preservative surgery of the foot, that important question, in my opinion, not being sufficiently settled, and the subject of my communication not having been discussed by the Surgical Society in Ireland. In such a Society as this, the Surgical Section of our Academy, made up chiefly of hospital surgeons and practical anatomists, it would be superfluous to enter with any minuteness into the anatomy of the joint of the ankle; but as we have amongst us some practitioners who have not the constant opportunities some possess of keeping up their anatomy, and in order to further elucidate the subject of injuries to the joint of the ankle, I shall briefly introduce my cases with a few practical remarks on the surgical anatomy of the ankle-joint.

The joint of the ankle is described as a perfect angular ginglymus; the bones are beautifully adapted, and compared to a tenon and mortise joint, from the closeness of their fitting. Three bones contribute to the formation of the ankle-joint—viz., tibia and fibula, which by their union form a deep depression, into which the upper surface of the astragalus fits. The tibia as it approaches

the ankle-joint loses its prismatic shape, and assumes a well-defined cubical or quadrangle form. On the lower surface of the tibia is a quadrilateral articulating cavity covered with cartilage; on the external surface of the tibia is a depression for the fibula, and the inner side is prolonged downwards for nearly an inch, and forms the malleolus internus.

The fibula as it approaches the ankle-joint becomes suddenly enlarged, and forms the malleolus externus.

The astragalus enters into the formation of the ankle-joint by its superior surface and a portion of its lateral surfaces. The articulating surface for the tibia is of an oblong quadrilateral form, and measures an inch and a half antero-posteriorly, and about an inch and a quarter transversely; the measurement is greater in front than behind (an arrangement which guards against dislocation of the tibia forwards and of the foot backwards). The mortise cavity is formed by the lower end of the tibia, and is completed by the fibula. The powerful ligamentous connection between the tibia and fibula makes the mortise very strong.

The *ligaments* are five in number:—

(1.) The *internal tibio-tarsal*, internal lateral, called the deltoid by Weitbrecht.

(2.) *Anterior tibio-tarsal* of Cloquet, very loose.

(3.) *Anterior fibulo-tarsal*, ligamentum fibulæ anterius of Weitbrecht, anterior external-lateral of Boyer.

(4.) The *middle fibulo-tarsal*, ligamentum fibulæ medium perpendiculare of Weitbrecht, external lateral of Cloquet.

(5.) *Posterior fibulo-tarsal ligament*, ligamentum fibulæ posterius (Weitbrecht), posterior external lateral of Boyer, also called the oblique ligament of Weitbrecht (strongest).

The *synovial membrane* of the ankle-joint is of very great extent; it is very loose upon the anterior and posterior surfaces of the joint, and is said to contain a greater amount of synovia than any joint in the body.

The *leg and foot* meet at a right angle in the ankle-joint; the fibula plays no part when the joint is at rest; the tibia alone receives the weight of the body and transmits it to the astragalus.

The *motions of flexion and extension* are the only ones permitted at the ankle-joint. In *flexion* the astragalus rolls from before backwards in the tibio-tarsal mortise; in flexion the foot and leg can form an angle of about 60° at this point; by the formation of the joint further flexion is prevented.

In *extension* the foot can be made to form an obtuse angle of about 150°; motions called abduction and adduction are not movements in the ankle-joint, but take place in the joints of the tarsus.

Winslow, who has given the most perfect description of the different motions of the foot, affirms that flexion and extension are the only movements permitted in the ankle-joint. Flexion and extension are the visible movements of the tibio-astragaloid joint.

The *tibia and fibula* form together a cavity which receives the pulley-like surface of the astragalus, and thus presents one of the purest hinge-joints of the human body.

Lateral motion is prevented.

The *buttresses* formed by the malleoli guard against luxations.

The *external malleolus* projects lower and more posteriorly than the internal, and in this way gives considerable strength to the joint by "wedging" the astragalus.

When we reflect on the great strength of the ligaments which connect the astragalus with the tibia and fibula, and the great support which the articulation derives from the prolongation downwards of the malleoli, we can easily perceive that a complete luxation of the ankle-joint can only be produced by great violence. Rupture of the ligaments, fracture of the malleoli, and protrusion of the bones of the leg render such accidents very complex.

Notwithstanding, however, the perfect construction of the ankle-joint, the numerous ligaments which bind the bones together, the strong tendons, their sheaths, and the different layers of fascia which greatly contribute to the solidity of the joint, violent accidents set all these precautions of nature at defiance, and produce the most painful and formidable displacements. Hancock says of the human foot, "In the whole range of mechanics, architecture, or engineering where can we meet with such a structure as this?" And referring to the study of the anatomy of the human foot, the

same distinguished surgeon and anatomist says, "That which was the student's former bane becomes his present delight, until at length, from contemplating the comprehensiveness of the design, the ingenuity and at the same time the simplicity, the wondrous adaptation of the several parts to their several functions, and withal the perfection and completeness of each individual part in itself, he cannot but feel the great responsibility which he undertakes in practising the surgery of the foot, and the impropriety, I am almost tempted to say the sin, of unduly sacrificing any portion thereof."

Nature has thus made ample provision for guarding against complete luxations of this joint—*firstly*, by the shape of the end of the tibia and the upper surface of the astragalus; *secondly*, by the malleoli grasping the astragalus; and, *thirdly*, by the attachments of the powerful ligaments supported by the numerous tendons, which in themselves act as ligaments. I have frequently endeavoured, when demonstrating the joint of the ankle in the dissecting-room attached to this College, to forcibly separate the bones of the leg from the foot, and the astragalus from the os calcis, in order to impress upon my pupils the enormous violence which must occur in order to produce complete luxation.

Case I.—*Compound Luxation of Ankle-joint, Tibia, and Fibula forwards.*—Mr. James Prosser, a farmer, residing at Tibradden, Co. Dublin, aged about thirty years, was sitting on a mowing machine in the month of August, 1872. The horse ran away, and Mr. Prosser's right foot got entangled in the machine, causing a compound luxation of the ankle-joint. The patient was seen shortly afterwards by my father, who telegraphed for me to come prepared to amputate a foot. I drove as quickly as possible to the patient's house, and on examination I observed the tibia and fibula protruding through the soft parts of the front of the right ankle; some of the tendons were lacerated, others stretched across the protruded bones; there was not much hæmorrhage. On consultation we decided against amputation, the patient being a fine strong countryman and of temperate habits. An anæsthetic having being administered, and the joint and protruded bones washed with a solution of carbolic acid, the dislocation was reduced, and the limb placed in a suitable splint. Subsequently the limb became enormously swollen, and there was much tension of the soft parts, necessitating numerous

free incisions to relieve tension. Subsequent treatment was carried out by my father, and I saw the patient occasionally with him, and we had the gratification of seeing our patient make a splendid recovery, with a useful foot. I have seen Mr. Prosser recently and examined his foot; he walks as well as ever, and follows his usual occupation as a farmer. The joint of the *ankle* is stiff, but there is compensatory motion in the medico-tarsal joint. I had the following letter from Mr. Prosser in March, 1889:—

"Tibradden, *March* 21*st*, 1889.

"Dear Surgeon Croly,—As you may remember, about seventeen years ago I met with a very sad accident, a compound dislocation of the ankle, which you came out to amputate, and afterwards yourself and your father saved, and now it is just as good and useful a leg as the other, I can walk and do my work the same as ever, with the exception of a stiff joint.—I remain your sincere friend, "James Prosser."

Case II.—William Nolan, a coal porter of intemperate habits, aged forty-eight years, residing at No. 2 Peterson's-lane, was admitted into the City of Dublin Hospital on the evening of the 29th of May, 1888, suffering from compound dislocation of the right ankle-joint. This accident occurred also at Rathfarnham, and the man gave the following account of the occurrence:—He was going up a short, steep hill, with his coal-car heavily laden, leading his horse by the head. The horse fell, bringing Nolan to the ground; his foot got caught between the horse's shoulder and the shaft, and as the horse endeavoured to get up Nolan's foot was forcibly wrenched outwards. He was seen very soon by my father, who sent him at once to hospital. On examination of the injured limb, I observed the tibia projecting through the soft parts for about 3 inches; the foot was much everted, and the fibula comminuted; there was not much hæmorrhage; the tip of the inner malleolus was detached; the astragalus was not injured. Ether having been administered by Mr. Jackson, house surgeon, the dislocation was reduced, the leg having previously been flexed on thigh, and thigh on abdomen; a large drainage-tube was inserted, and the limb placed in suitable splints and dressed antiseptically. Opiates, with bromides, were administered to soothe the nervous system. The following day the tension of the limb was very great up to the knee, necessitating numerous free incisions. Six small fragments of fibula came away through opening on fibular side. November

26th following, I removed 2 inches of the end of the tibia, which became detached. The patient is now able to put his foot to the ground, and there is every promise of his having a very useful foot.

Case III.—Thomas Smith, of Williamstown, aged twenty-eight years, of very temperate habits, was admitted into the City of Dublin Hospital on Sunday morning, 4 o'clock, 8th July, 1888, suffering from compound luxation of the left ankle-joint. (This case is very like No. 19 of Sir A. Cooper.) He gave the following account of the accident:—He went to Merrion Farm, and was lying on a bench of straw, covered by an iron roof, intending to sleep there, so as to do some extra early work in the morning. A bundle of the straw which he was on gave way, and he fell to the ground, a distance of 20 feet. He was stunned by the fall, and on recovering his consciousness he tried to walk, but found he could not do so, and saw the bones protruding through his boot. He was conveyed at once to the City of Dublin Hospital. I was telephoned to the hospital about the case at 4 a.m. on Sunday morning, and on arriving there I observed the tibia and fibula protruding more than 3 inches through a small opening in the soft parts at the outside of the joint. There was no fracture of the bones; the inner edge of the foot was turned upwards, almost touching the inside calf of the leg (*see* drawing). There was very little hæmorrhage. The patient having been placed under the influence of ether by Mr. Jackson, house surgeon, and the leg flexed on thigh, and thigh on abdomen, I endeavoured to reduce the dislocation, but found it necessary to enlarge the wound upwards on the fibula. The protruded bones and soft parts having been well washed with a solution of carbolic acid, reduction was easily effected, and the limb placed in suitable splints, the wound closed and dressed antiseptically; there was considerable tension of the limb in this case, also necessitating free incisions. The patient made an uninterrupted recovery, and the wound healed rapidly, and in three months the patient was able to put his foot under him and move about the ward, and very shortly afterwards was able to leave hospital and resume his work, having a perfect foot.

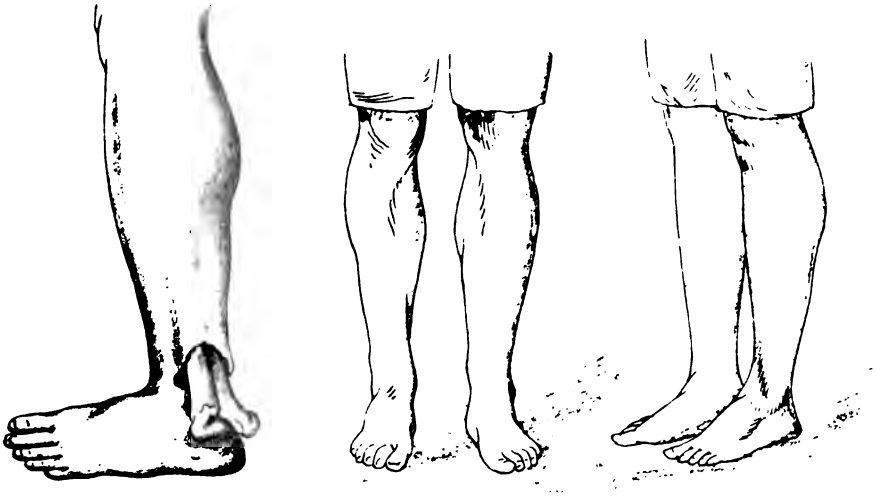
"8 Castle Dawson-avenue, Williamstown,
"*December*, 1889.

"I can walk, and run, and work as well with my left foot, injured in June, 1888, as ever I did, and am not one bit lame.

"Thomas Smith."

Case I.

Case II.



Case III.

MR. CROLY ON COMPOUND LUXATION OF THE ANKLE-JOINT

Falconer Dublin.

Sir A. Cooper, who devoted more time to this subject than any other surgeon, has collected some very valuable information on the subject of compound dislocation of the ankle.

"Having endeavoured," says Sir A. Cooper, "to explain what has fallen under my own observation and what I have been able to learn from others upon this difficult subject, I beg leave to express a hope that any of my friends who may have had cases under their care which would throw further light upon this subject will have the kindness to communicate them to me, whether they make for or against the advice I have given, *videlicet*, 'that amputation is not generally necessary in compound dislocation of the ankle,' as I have no further wish but that all the points respecting this severe accident may be fully elucidated and established."

Reviewing the history of the treatment of compound luxations of the ankle-joint, I find that Hippocrates removed the ends of the leg-bones for compound dislocation; but it was not until the end of the last century that the practice became generally recognised. During the interval the limbs of patients suffering from these accidents were invariably sacrificed. The honour, however, of introducing this great improvement (preservative surgery of the foot) into surgery is undoubtedly due to Mr. Hey, of Leeds, who in the year 1776 sawed off the end of the tibia of a man who had been tossed by a bull and sustained compound luxation of the right ankle. The patient recovered and had a useful foot. Mr. Hey was strongly in favour of trying to save the limb in compound luxations of the ankle. In 1767 Gooch removed the tarsal extremity of the tibia with success, and cases are also recorded by White, of Manchester, in 1770, and by Servius in 1778. In 1782 the elder Moreau, nineteen days after compound luxation, sawed off the articular end of the tibia; the patient recovered completely, with the use of the new joint. The operation then remained in abeyance until 1799, when it was again performed by Hey.

Roux gives much praise to English surgeons for the judicious boldness which they have evinced in the treatment of compound luxations of the ankle-joint, by reduction and removal of the ends of the bones when necessary. Roux acknowledges that the bold

practice of sawing off the ends of the bones in compound luxations of the ankle originated with and was first executed by English surgeons. It is evident, therefore, that Sir A. Cooper was not the first surgeon to recommend saving the limb in cases of compound luxation of the ankle. Yet he deserves much credit for having collected such a number of cases of this injury, and also for having taken such a decided stand on the side of preservative surgery.

Hancock, in his beautiful work on the anatomy and surgery of the human foot, says, "I will pass on to those alarming accidents in which, through accident, the joint of the ankle is laid open and its internal economy exposed. Here I shall have the opportunity of relating some of the most brilliant achievements of modern surgery;" and referring to the course of practice to be adopted in such serious accidents, the same distinguished writer says, "No general precepts can guide the surgeon in this delicate question; genius alone cannot do it; the opportunities of making observations and the talent of profiting by them are here the things which make the consummate surgeon."

Ashurst says, "Compound dislocations of the ankle are accidents of extreme gravity, as is well shown by the number of fatal cases which are on record. In deciding upon the course of treatment we are to be guided to a certain extent by the age of the patient; it is the danger to life, rather than the question of usefulness, which must chiefly guide us in determining what course to pursue. As to pure conservatism, the majority of cases in which simple reduction has been practised have resulted disastrously, with suppuration and caries, leading to pyæmia; hence the precept that primary amputation or excision should be performed. The *perfection of antiseptic surgery* has, however, raised anew the question of conservative treatment."

Agnew says, "At one time amputation was deemed the only proper measure in a case of this nature; a larger experience, however, has shown that in most cases of this injury a more conservative course may be adopted with a reasonable prospect of saving the limb."

Erichsen says, "The treatment of compound dislocation of the

ankle-joint must depend to a considerable extent upon the laceration of the soft parts and the condition of the bones forming the joint. If the wound to the soft parts be moderate in extent—clean cut and little bruising and injury to the bones—an attempt should be made to save the limb. This is to be done by the *assiduous use of antiseptics.* In many instances the patients will recover with a stiff but useful limb, the joint being only partially anchylosed; if, however, the bones be projecting and comminuted, and the soft parts extensively lacerated, the question of amputation will necessarily arise." Mr. Erichsen adds, "I believe that the disinclination on the part of the surgeon to amputate in these cases is greatly owing to the strong expression of opinion by Sir A. Cooper, in favour of the attempt to save the limb, having, in many cases, been carried to such an extent as seriously to add to the patient's danger. Secondary amputation may be necessary in consequence of gangrene, erysipelas, or extensive suppuration."

That accomplished surgeon and anatomist Sir William Fergusson says, "In the example here represented" (he pictures a case of compound luxation of the ankle-joint where the tibia and fibula were thrown over on the neck of the astragalus) "I amputated the foot. The operation was not successful, and the practice may seem very questionable." Sir William adds, "Whether the patient would have had a better chance of life with his foot on, it is impossible to say, but it would appear that under any circumstances amputation in cases of the kind is far from being successful." Fergusson then quotes from the *Medical Journal* for August, 1854, and says, "It has been stated by Syme that out of thirteen amputations performed in the Royal Infirmary for compound luxations of the ankle only two recovered, an amount of mortality which may well incline the surgeon to act upon the doctrines inculcated by Sir A. Cooper."

Percival Pott, in referring to compound luxation of the ankle, says, "These cases not infrequently end in fatal gangrene unless prevented by timely amputation, though," he adds, "I have several times seen such cases do well without."

Bryant says, "The treatment of compound dislocation of the

ankle-joint cannot be reduced by any definite rules. Each case must be treated on its own merits. When a small wound exists operative interference is only exceptionally needed, but when a large one, with projection of the bones, it is a question whether the better practice lies in the reduction of the dislocation after cleansing the projecting bones, or in their resection. When the bones cannot be reduced by ordinary force it becomes a necessity; when the bones are much crushed their resection should always be undertaken; indeed, it is a general feeling in my own mind that in compound dislocation, as in compound fractures with a large wound, it is wiser to resect the ends of the projecting bones than to reduce them. Amputation of the foot should only be performed when the soft parts are much injured, and the age of the patient or his power forbids the hope of recovery with a useful limb being secured."

That excellent practical surgeon the late Mr. Stapleton records, in the *Dublin Hospital Gazette*, a case of compound luxation of the ankle-joint in which the tibia protruded and the internal malleolus was broken off; the fibula was fractured in two places. The case terminated most favourably. There was little or no suppuration. Mr. Stapleton attributes the rapid recovery in a great measure to the very abstemious habits of the patient.

Three courses are open to the surgeon in cases of compound dislocations of the ankle-joint, *videlicet*—(1) reduction; (2) sawing off the ends of the bone, and then reduction; (3) amputation.

Reasons for Excising Ends of Bone.

1. Difficulty in effecting reduction.
2. If the fracture at the end of the tibia is oblique, which would prevent the fractured portion from remaining on the astragalus.
3. To relieve spasm of the muscles.
4. To diminish local irritation and hasten cure.

Cases in which amputation may be necessary either to save Life or to prevent the Patient being doomed to Crutches.

1. Advanced age.
2. Very extensive wounds caused by machinery, or extensive

contusion of the soft parts by a heavy-laden carriage passing over the limb.

3. Shattered bones of ankle and fractured astragalus and os calcis.

4. Severe hæmorrhage, caused by wounds of tibial arteries and veins.

5. Threatened mortification of foot.

Practical Conclusions.

1. In compound luxations of the ankle-joint there is usually also fracture of one or both malleoli, with laceration of the ligaments.

2. If the end of the protruded bones are fractured obliquely, or spiculated, or covered with sand or dirt, or the periosteum detached, the end or ends of the bones should be resected.

3. To effect reduction the patient should be placed under the influence of an anæsthetic.

4. Flexion of leg on thigh and thigh on pelvis should *always be adopted*, to relax the muscles and facilitate reduction.

5. If necessary the wound should be enlarged to facilitate reduction, with or without excision of the ends of the bones.

6. Antiseptic dressings should invariably be used.

7. Drainage-tubes should be inserted, to facilitate discharge.

8. Suitable splints should be adjusted, and avoidance of tight bandaging.

9. Opium or chloral with bromide should be administered to quiet the nervous system, especially in persons addicted to strong drink.

10. The diet should be light and unstimulating.

11. It is very remarkable that Sir A. Cooper makes no allusion to the swelling and tension of the limb in cases of compound luxation of the ankle-joint, nor does he refer to the *urgent need of free incisions*. The same applies to other surgical writers. In all of my cases incisions were urgently needed. If tension of the limb sets in (which occurred in each of my cases), as indicated by swelling and formation of bullæ, free incisions should be made from the

ankle to the knee, to relieve tension, to give exit to serous and purulent fluids, and prevent gangrene.

12. If purulent discharge continues for a long time from the ankle-joint loose pieces of bone may come away or require removal.

13. Experience shows that limbs doomed to primary amputation have ultimately recovered perfectly.

14. If amputation should be necessary, secondary amputation is much more successful than primary.

15. These cases are not suitable for Syme's amputation at ankle-joint, as gangrene is almost sure to set in, owing to the lacerated and contused state of the soft parts.

CASES OF COMPOUND DISLOCATION OF THE ANKLE-JOINT,

—	Sex	Age	Nature of Displacement	Treatment	Result as to Use of Foot, &c.	Name of Surgeon	Observations
1	M	—	Tibia	Reduction	Recovery. Useful foot	—	Amputation advised. Patient and friends refused. Another surgeon put in charge.
2	M	32 yrs.	Tibia and part of astragalus inwards	Reduction. Astragalus excised	"	Mr. Lynn	None.
3	M	Adult	Tibia inwards	Tibia reduced	" (stiff ankle)	Mr. Battley	Patient at work in 9 months. Foot good as ever. Case seen by Sir A. Cooper at end of 3 years.
4	F	48 yrs.	Tibia inwards. Fibula fractured	Tibia reduced	Recovery. Useful foot	Mr. Richards	Patient walked without stick.
5	F	Adult	Fibula *outwards*. Tibia fractured	Reduction	"	Mr. Rowley, St. Thomas' Hospital	Patient walked in 4 months.
6	M	22 yrs.	Tibia inwards 2 inches. Fibula badly fractured	"	"	Mr. Clarke	Patient wrote to Sir A. Cooper 4 years after the accident saying he could walk 3 or 4 miles easily, or 8 if required. Would not exchange his leg for a wooden one for the whole of Europe.
7	—	—	Tibia inwards	"	"	Mr. Somerville, Stafford Infirmary	
8	—	—	" "	"	"	"	
9	M	38 yrs.	Tibia and fibula *outwards*. Astragalus inwards	"	"	Mr. Scarr	Patient convalescent in 25 weeks. Foot very useful in a year. This patient was sent to London and seen by Sir A. Cooper, who had thus an opportunity of witnessing the happy result of Mr. Scarr's skill and judgment.
10	M	72 yrs.	Tibia inwards, also fractured. Four inches protruding, covered with sand. Joint filled with sand and blood	"	"	Mr. Abbott, Needham Market	Patient a butcher; corpulent, intemperate, gouty from youth. Amputation proposed. Patient refused. Walked well in a year. Lived 10 years, and walked as well as ever.
11	F	45 yrs.	Tibia inwards. Fibula protruded. Cavity of joint opened	Portion of bone removed. Reduction	"	Mr. Rawson, Manchester	Perfect recovery.
12	M	36 yrs.	Tibia inwards	Reduction	" (stiff ankle)	Mr. Chandler, Kent Co. Hospital	
13	M	—	Tibia	Not reduced	Recovery. Useful foot	Mr. Wickham	Case neglected before Mr. Wickham saw patient. Got well ultimately.
14	M	60 yrs.	Tibia inwards	Reduced	"	Mr. Fiske	
15	M	Adult	Tibia *forwards*. Fibula fractured	Reduction	"	Mr. Maddock	Case most unfavourable on account of extensive laceration of ligaments and tendons.
16	M	Youth	Tibia and fibula *outwards*	"	"	"	Loose portions of fibula removed; abscesses formed up leg. Recovery.
17	M	"	" " "	"	"	"	

TABLE—*continued.*

—	Sex	Age	Nature of Displacement	Treatment	Result as to Use of Foot, &c.	Name of Surgeon	Observations
18	M	13 yrs.	Tibia and fibula outwards 4 inches. Astragalus loose and torn from its connecting ligaments. (Machinery accident.)	Reduction	Recovery. Useful foot	Mr. Ormond	Amputation postponed in this very bad case on account of the shock. Severe inflammation extended up thigh. Operation *again* postponed. Extensive sloughing took place. Four inches of end of fibula exposed. In 15 weeks the boy walked half-a-mile with the aid of crutches, and soon laid his foot flat on the ground and walked without the aid of a stick. Mr. Ormond sent Sir A. Cooper a piece of this boy's tibia which exfoliated, and Mr. Ormond says he does not claim the merit of saving the boy's foot, as he merely postponed the amputation on account of shock and subsequent inflammation.
19	M	13 yrs.	*Tibia and fibula outwards*	Ends of tibia and fibula excised. Reduction	,,	Sir A. Cooper, Guy's Hospital	This boy was able to bear on his foot in 4 months; walked well. Sir A. Cooper, who was urged to amputate this boy's foot, said he could not doom the lad to a life of mendicity, and determined to try and save the limb. He had the inconceivable pleasure of seeing this boy, in 4 months, walk before the students from one end of the ward to the other.
20	M	40 yrs.	Tibia inwards. Integuments nipped into joint. Foot loose	End of tibia excised. Reduction	,,	,,	
21	M	38 yrs.	Tibia inwards. Fibula fractured	End of tibia excised. Reduction	,,	Mr. Forster, Guy's Hospital	
22	M	40 yrs.	Tibia and fibula outwards. Astragalus fractured. Superior half of astragalus attached to the bones of the leg, also oblique fracture of upper part of femur.	Reduction	,,	Mr. Rumsey, Aversham	Amputation not done on account of the complication of fractured femur.
23	M	Adult	Tibia and fibula inwards. End of fibula fractured	1½ inches of bone excised. Reduction	,,	Mr. Hicks	
24	M	36 yrs.	Tibia and fibula outwards. End of fibula fractured longitudinally for 3 inches	Ends of bone excised. Reduction	,,	Mr. Cooper, Bradford	Sir A. Cooper saw this man 2 years after the accident. He could go up and down a ladder, and followed his business as a painter as well as ever.
25	M	15 yrs.	Tibia. Anterior tibial artery wounded.	End of tibia excised. Reduction. Anterior tibial artery tied	,,	Mr. Sandford, Worcester Infirmary	
26	M	Adult	Tibia inwards	End of tibia excised	,,	Dr. Lynn, R.N., "Walmer Castle," East Indiaman	
27	F	72 yrs.	Tibia inwards. Fibula fractured	Reduction	,,	Mr. Needham, Leicester Infirmary	
28	M	18 yrs.	Tibia inwards. External malleolus fractured	End of fibula excised. Reduction	,,	Sir A. Cooper, Guy's Hospital	

			naviculare. Fibula fractured 2 inches above malleolus				and several cases of compound luxation of the ankle-joint under his care, and the results of his observations have been that in such cases there is not only a chance of saving the limb, but of that limb being at a future time useful. Mr. Hammicle relates a case in which the lower end of the tibia became carious, and after 18 months 3 inches of the bone came away.
30	M	14 yrs	Tibia and fibula outwards. Ends of malleoli fractured	"	"	Mr. Bryant, Guy's Hospital	
31	M	—	Tibia projected	End of bone removed	"	Mr. Bickersteth, Liverpool Royal Infirmary	
32	M	1 yr. & 9 ms.	Tibia forwards. Anterior tibial artery wounded	Reduction	"	Mr. Budd and Mr. Waldron, Worcester Infirmary	
33	M	38 yrs.	Tibia inwards	End of tibia resected. Reduction	"	"	
34	M	32 yrs.	Tibia and fibula forwards	Reduction	"	Mr. Greenhow, Leeds	In 2 months wound healed and patient moved about on crutches.
35	F	10 yrs.	Tibia and fibula outwards. Foot turned upwards at right angles	Ends of bones resected 1 *inch*, to facilitate reduction	"	Mr. Garraway	Enough of tibia and fibula removed to prevent contact with the astragalus, thus saving the ankle-joint.
36	M	8 yrs.	Ankle-joint completely opened from accident	Reduction	"	Mr. Wheelhouse, Leeds Infirmary	Amputation agreed upon. Boy placed on table. Decided then to give him a chance on account of his age. Perfect recovery.
37	F	—	Tibia and fibula projecting. Foot inverted to a right angle Joint completely opened	"	"	Mr. Thompson, of Launceston	Portion of dead bone came away. Patient walked briskly.
38	M	Adult	Tibia inwards 4 inches. Fibula fractured	End of fibula resected. Reduction	"	Mr. Parrott, Enfield	Two years after accident patient could dig; foot as strong as before the accident.
39	—	—	Tibia and fibula	Ends of bones resected. Reduction	"	Gooch	
40	M	30 yrs	Fibula 2 inches. End comminuted. Tibia fractured at malleolus	End of bone resected	"	Lister	In 6 weeks the fracture of the tibia united, and the patient could move the ankle-joint.
41	M	Adult	Tibia inwards. Fibula fractured in two places	Reduction	"	Mr. Stapleton, Jervis Street Hospital	Little or no suppuration; patient abstemious.
42	M	Adult (*private patient*)	Tibia and fibula forwards projecting on dorsum of foot. Machinery accident, tendons torn. Whole front of joint opened	"	"	Henry Croly, M.D., F.R.C.S., Rathfarnham, and Mr. Croly	This patient, a farmer, had diffuse inflammation of leg, requiring free incisions. Case under treatment about 9 months; recovered with useful foot.
43	M	48 yrs.	Tibia inwards 4 inches. Fibula comminuted. End of tibia fractured. Several portions of bone extracted. Admitted to Hospital, May 29th, 1888	Reduction under ether	"	Mr. Croly, City of Dublin Hospital	Patient, coal porter; hard drinker. Inflammation of leg followed from tearing and infiltration of the muscles. Free incisions made up the leg. Several small portions of fibula came away, also articulating end of tibia, 6 months after accident. Foot firm. Patient still under treatment.
44	M	28 yrs.	Tibia and fibula *outwards*, torn completely from ligaments. Foot completely *inverted*. Inner side almost touching side of calf of leg	Wound enlarged. Reduction under ether	"	Mr. Croly, City of Dublin Hospital	This patient never tasted whisky. Naturally strong young man. Made a rapid recovery. Walked up and down ward before students in 3 months. I exhibited him at the opening meeting of the Surgical Section of the Royal Academy of Medicine, Nov., 1888.

Cases in which Amputation was Performed, or where Death Occurred without Operation.

—	Sex	Age	Nature of Displacement	Treatment	Result	Name of Surgeon	Observations
1	M	Adult	Tibia inwards. Extensive laceration of soft parts. Severe hæmorrhage	Amputation	Recovery	Sir A. Cooper	
2	M	36 yrs.	Tibia inwards. Machinery accident	"	"	"	Operation 7 weeks after accident.
3	F	34 yrs.	Tibia and fibula outwards. Caused by jumping from a window. Astragalus protruded	"	"	"	Operation on 16th day.
4	M	48 yrs	Tibia and fibula outwards. Caused by wheel of a cart. Os calcis also fractured	"	"	"	
5	M	Adult	Tibia inwards. Astragalus dislocated	Tibia reduced. Astragalus removed. Foot amputated in 2 years	"	Mr. Norman, Bath	
6	M	"	Tibia inwards. Fibula fractured	Reduction	Death	Sir A. Cooper	Died 8th day. Septicæmia.
7	M	—	Tibia and fibula forwards	Amputation	"	Sir Wm. Fergusson, King's College Hospital	Sir William Fergusson says: "Whether the patient would have had better chance of life with his foot on it is impossible to say, but it would appear, under any circumstances, amputation in cases of the kind is far from being successful."
8	M	Adult	Tibia and fibula forwards. No fracture	"	"	—	
9	M	"	Tibia inwards	"	"	Recorded by Chelius	In this case bones were reduced, soft parts sloughed, patient became delirious, and during violent efforts the bones were thrust through the mortified skin. Amputation performed and case ended fatally.
10	M	Old Man	Tibia and fibula forward	"	"	Mr. Croly, City of Dublin Hospital	

A SUCCESSFUL CASE OF BATTY'S OPERATION.

By JOHN K. BARTON, M.D., F.R.C.S.;
Surgeon to the Adelaide Hospital.

[Read in the Section of Surgery, December 6, 1889.]

Of all the morbid conditions which the surgeon has to contend against none are so difficult to deal with as what has been called the hysterical condition. There is something at once so vague and, at the same time, so changeable that the name conveys to our minds no very definite idea, but rather a suspicion that the patient is shamming. Thus, if we are assembled round a bed examining a knee said to be the seat of disease, the moment the word "hysterical" is pronounced all turn away, as if the disease had vanished or become unreal, and at the same time treatment was at once useless and unnecessary; but, although the conditions are changed, yet disease remains, and that, too, more difficult of management than before. Changes due to inflammatory action are slight. Temperature may be normal, or nearly so, but at the same time the group of reflex signs of irritation are greatly increased, so as to assume the importance of a disease. Thus, in the following case reflex vomiting became so severe, so obstinate, so complete that the patient sank to a very low state, and was for a long time supported only by nutritive enemata. The signs of inflammatory action were slight, but the reflex phenomena threatened the extinction of life. To me it seems that the hysterical condition adds greatly to the difficulty of the case, and demands closer study than it has yet received to enable us, when it is found complicating disease or injury, to turn at once to the proper line of treatment, so as to counteract that morbid sensitiveness which produces vomiting and reflex symptoms of all kinds, and which seems to upset the healthy balance of the mind itself, producing an exaggera-

tion of symptoms, a craving for pity, and such a desire for sympathy that truth is quite overlooked in its pursuit.

Now, when this morbid condition of the nervous system exists, or seems to exist, without any disease, it more properly comes under the care of the physician; but when it complicates, as it does in the majority of cases, an already existing disease—frequently inflammatory—then the nature of the preceding disease may place the case under the hands of the surgeon, and he will find it difficult to make out accurately the signs of the disease, marked as they are by the exaggerating reflex phenomena.

The reason which has induced me to preface the case I am about to relate with these remarks upon the complicating hysteria is that it seems to me that the result in this case aids us in forming a sound opinion as to the treatment which such hysteria demands—viz., *the removal of the exciting cause.* In all cases of hysteria there must be *an* exciting cause, but in such cases as this we know *the* cause whence the current of nervous irritation begins. Remove this and we cure the hysteria—all else is mere palliation; sometimes worse than useless. But a very curious phenomenon, illustrated also by this case, is the fact that the reflex hysterical symptoms do not *at once* cease when the cause is removed, but gradually disappear, sometimes starting up again, as if all had been in vain, but quickly subsiding and finally disappearing.

CASE.—Mary M., aged twenty-six, was admitted into "Brooke" ward, Adelaide Hospital, on the 9th of October, 1888, complaining of an aching pain in the left side of the abdomen. This pain began a year ago, when returning from a long walk. She shivered and fainted, and when recovered from this she felt the pain for the first time, which had continued ever since, sometimes being of a shooting, darting character, extending to the back of the hips; at other times it is of a throbbing, burning character—but always present.

Family History.—Father died of consumption when patient was an infant. Mother, struggling to maintain herself and child, died of a *tumour* twelve years ago; no brothers or sisters; has been supporting herself as a servant of all work.

Present Condition.—A rather stunted, undergrown girl, with a furtive expression of countenance. The abdomen is swollen and

prominent, tympanitic on percussion; in the left hypochondrium pressure causes her to wince, and when asked to point out the seat of the constant pain of which she complains she points to a spot two inches inwards from the spine of the ileum, and on a line with it, and adds that this pain frequently extends round to the outside and back of the hip on that side; when deep pressure is made on the place she indicates she complains greatly, and there can be no doubt suffers sharp pain. The left leg is the seat of pain, like cramp, and she states that she is not able to walk properly from the weakness of this leg. The catamenia are now regular and moderate, but have until lately been profuse, and appearing at intervals of two weeks. Bowels have been too free, diarrhœa being very easily provoked. Micturition has been frequent and very painful; sometimes she found it very difficult and slow. Her sleep seems very broken, and perspiration at night common. Her temperature on the day of her admission was 98·6°; her pulse, 70.

The following night, however, her temperature was 100·2°, and she had a restless night. The pain over the left ovarian region was more marked. A blister was applied over this spot, of the size of a half-crown piece, and kept on until it rose well. This day (that is, the day after her admission) she vomited a greenish fluid, but took her food through the day. Careful records have been kept of this case by Miss Harper, from day to day, from which I will summarise.

The diagnosis which was made was "inflammatory disease of the left ovary." How far this had gone—whether producing suppuration or not—there was as yet no means of saying; but the disease was plainly complicated with extensive hysterical symptoms, of which constant vomiting was the most serious. Sometimes a small blister over the left ovarian region was followed by relief to the vomiting, but when repeated again no relief whatever resulted. Another most distressing symptom was flatulent distension of the abdomen. This was always a sign of the irritation being worse, and was strangely variable—sometimes the abdomen was distended so as to look as if an enormous ovarian tumour was present, and soon afterwards not half the size.

As an illustration of her state I may take the note recorded upon January 5th:—"Patient was very much troubled by flatulence all yesterday, not being able to lie down for more than a few minutes at a time. Last night she was given a hypodermic of morphia, and slept very well after it. She vomited this morning.

Her abdomen is a good deal distended and tympanitic, and pain is very bad at intervals. E. T., 99°; M. T., 98·8°."

On the 8th we read:—"Patient did not sleep at all last night on account of a severe pain in her back and left side. She vomited all through the day. This morning has taken a cup of tea, which she threw up at once. Her abdomen is greatly distended."

Her food at this time was cold chicken jelly, the only thing I found would be retained.

On the 25th of same month the note was:—"Patient was very ill all day yesterday, vomiting continually. She is a little better this morning, but can take *nothing*. Her bowels have been moved three times to-day. Her abdomen is distended and tympanitic."

Nourishing enemata were now resorted to, as all drugs to arrest the vomiting had failed. I may mention here that the bromides were tried in free doses, and for a time had a beneficial effect, but very soon lost their power.

The question of operation had been frequently discussed, and as often postponed because the symptoms varied so much from day to day. For days it appeared as if she was getting well, and that the case was *all hysteria;* but as time passed, and she continued getting weaker and weaker, I decided that the operation of removal of the ovaries, and all the parts about them found to be diseased, was the only treatment which would succeed, and should therefore be done. The nature of the proposed operation was fully explained to the patient, who willingly embraced the hope which the operation afforded her of relief, counting the inability to become a mother as no loss in her case.

Accordingly, on the 5th of February, 1889, the operation was performed as follows:—A short incision (3½ inches) was made in the linea alba, commencing just below the umbilicus, and the peritoneum opened to same extent. The left hand, well antisepticised, was at once passed down into the pelvis on the left side, and the uterus felt. Then very quickly the ovary was caught and drawn up out of the wound. It was found to be enlarged to the size of a small walnut, but more elongated, and presented a puckered, whitish surface, as if of a cicatrix. The Fallopian tube and broad ligament were studded with minute cysts. The pedicle of the ovary was ligatured with softened trout-gut suture both near the uterus and near the ovary, and divided between them. The Fallopian tube was excised in a similar way, and the pedicle dropped back into the pelvis. The hand was then passed down into the right

side, and the ovary caught and brought up for inspection, when it was found to present also decided marks of inflammatory disease. It was therefore excised, and its Fallopian tube, in a similar manner to that employed on the left side ;[a] and then the peritoneal cavity was thoroughly washed with a very abundant douche of the boric acid (1 to 20). This having been accomplished, it only remained to close the peritoneum and the wound, which was done in the usual manner, no drainage tube being inserted. Corrosive dressings were employed; a large common gauze-pad over all, and a flannel binder supplementing it. The patient, who had seemed very weak twice during the operation, which lasted an hour, rallied very soon, but a distressing sickness from the ether continued until the following day, when it subsided, and she began to take food. Her temperature never was higher than 100° after the operation, and although it continued very variable, yet on the whole it gradually settled down to the normal line. The wound healed soundly, under antiseptic dressings, without any suppuration. For some weeks after the operation she had occasional pain in the left ovarian region, and once or twice violent reflex vomiting, which subsided very soon, thus showing a marked contrast to the sickness previously, which was most obstinate and quite unaffected by remedies.

In three weeks after the operation she was able to leave her bed, and in the following month left the hospital.

She came to see me lately, and reported herself well. She was looking for a situation as a servant. I may add that her appearance bore out her statement that she was now able for the hard work of an under-servant. She had a little of the furtive look of the hysterical patient, and spoke and laughed in the same way; but knowing how ineradicable is the hysterical diathesis, I feel certain that the operation has done more for her than I ventured to hope when I proposed it, which I did to arrest the train of reflex symptoms which had reduced her to the most pitiable condition of weakness, and to arrest which medicine had proved useless. Not only had "oöphorectomy" done this, but had in less than three months transformed her from a hopeless sufferer to a strong girl, able to take her part in life and earn her own bread. Some may,

[a] Dublin, Dec. 6, 1889.—The ovaries removed by Mr. Barton on microscopic examination showed an increase in the number and size of the blood-vessels, both arteries and veins. There were in places, especially near the blood-vessels, collections of small inflammatory cells (leucocytes). The whole appearance of the organs was that of chronic inflammation.—H. T. Bewley.

on theoretic grounds, and the interference with "the sacred rights of a mother," object to and make little of this proceeding. My patient has no doubt whatever as to the good it has conferred upon her, and finding herself restored from acute suffering and most debilitating vomiting to health and strength, she is most thankful to find that the price paid for this restoration is the impossibility of pregnancy, which, in her case, would mean a return of all her worst symptoms, probably with premature labour.

Whether this operation of "oöphorectomy" is rightly termed Batty's operation or not does not much concern us. I believe Mr. Lawson Tait was one of the very first who performed it, as he undoubtedly is now its foremost advocate in cases of dysmenorrhœa and menorrhagia. Agnew has collected the particulars of 171 cases in which this operation has been performed, of which 139 recovered and 32 died, being a death percentage of 18·72.

This table was drawn up ten years ago, so that we may safely assume that many of the cases were not done with antiseptic precautions, and that therefore this percentage may be and is very much less now.

The morbid conditions for which Batty's operation has been done are exceedingly varied. They comprise not only all diseases of the ovaries themselves, both organic and functional, but many uterine troubles, more especially uterine fibroids, where the accompanying menorrhagia—which is really the only urgent or even serious symptom generally—may be arrested at once and permanently by the removal of the ovarian and uterine appendages, the "sacred rights of the mother" theory not being applicable here—as pregnancy is not likely, if possible—and the operation is recommended as a safe alternative for hysterectomy.

As the case I have had the honour of recounting does not bear on this part of the subject, I will now conclude by pointing out that the good obtained in this may be safely taken as that which may be expected in all such similar cases, and this was restoration of a young woman from a life of constant pain and violent reflex vomiting, which deprived her of all strength or power to help herself in any way, to a state of good health, cheerfulness, and usefulness—the price paid for this being, from her point of view, a great gain.

SUPRAPUBIC LITHOTOMY IN RELATION TO THE TREATMENT OF ENCYSTED CALCULUS.

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[Read in the Section of Surgery, January 17, 1890.]

This is a subject which I believe deserves special attention. It derives its chief interest from the fact that it is possible, owing to the perfection of this method of lithotomy, to deal satisfactorily with calculi which can be reached, but cannot be safely removed, through a perineal opening.

The case which I am about to relate was one of so grave a nature, and so surrounded by elements of danger, that I do not hesitate to detail at some length the previous history, the operation, the course, and after-treatment.

Case.—The patient, Mr. C., aged forty-two, came under my care on the 5th of July, 1889, giving the following history:—Since 1874 he has had symptoms of stone, but until 1888 he was able to bear the inconvenience. Early in 1888 he was obliged to undergo a lateral lithotomy, which gave him only temporary relief, as some five months before he came under my care he had been subjected to lithotrity, which afforded him only momentary relief also. He came under my care in the following condition:—He was pale and pasty-looking, with œdema of lower lids and of legs; quick, irregular, intermittent pulse, and hurried respiration. Micturition was frequent and very painful, pain being referred to anterior half of urethra. The urine was loaded with phosphates, contained pus in large quantity, and when allowed to stand the supernatant fluid contained a large amount of albumen. He had frequent vomiting, and cold perspirations frequently broke out over the upper half of his body, more especially about his head and face. When he attempted to pass water he grasped the bed and became almost convulsed owing to the extreme torture he suffered on these occa-

sions. On examining the bladder, I found a small free calculus the moment the sound entered the bladder; but, as this was not enough to account for the great distress, with the sound still in the bladder, I made a rectal examination, and discovered behind, and to the right of the prostate, a hard nodulated mass, which, on firm pressure above the pubes, I made out to be ovoid in outline, with long axis antero-posterior, somewhat nodular, and very firm, and occupying the region between the right ureter and the posterior edge of the prostate, which it indented.

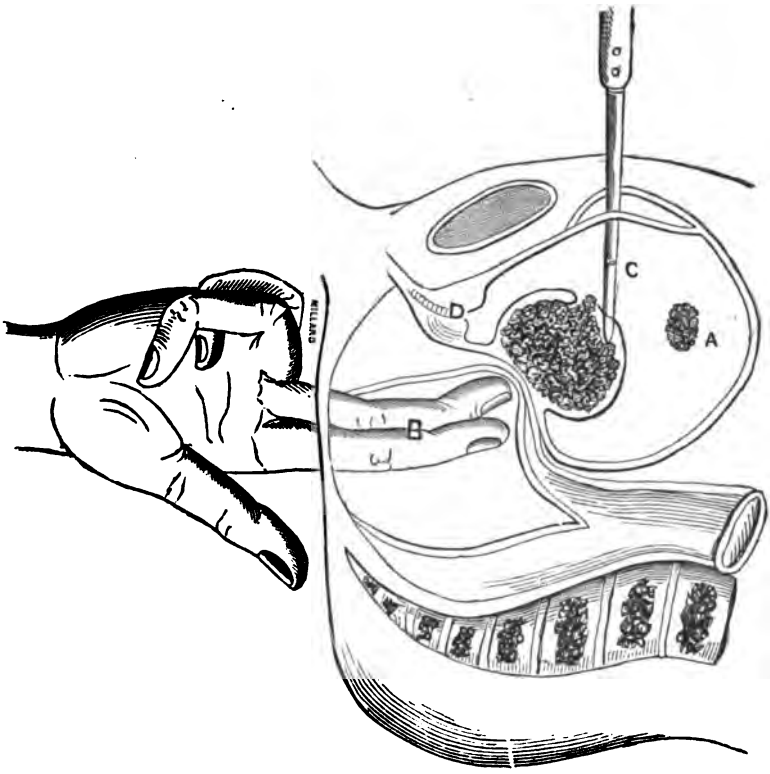
With the sound I could detect occasionally a point on the vesical side of this mass, which gave undeniable evidence of its being calculous.

On July 12th Sir William Stokes saw the patient with me, and after careful examination we decided on the suprapubic operation, and Dr. Cox, who examined the case for me, assured me there was no lesion of the kidney which contra-indicated surgical measures. The encouragement thus given by one whose opinion I value highly induced me to undertake a grave operation on a patient already so broken down.

Operation.—On July 13th the following operation was performed:—After introducing Petersen's bag, and distending it with 10 ounces of boracic solution, 8 ounces of a similar fluid were injected into the bladder, an incision 2½ inches in length was made above the pubes, laying bare the cellulo-fatty tissue, which I scraped through. Reaching the bladder I grasped it with catch forceps, and incised it. After allowing the boracic solution to flow I passed two suspensory sutures through each lip of the vesical wound. Enlarging the opening, I introduced my left index finger, and at once came upon the loose calculus (Fig. 3 A), which I removed with a small scoop. I now explored the right half of the base of the bladder, where I discovered a mass projecting into the trigone on that side, and corresponding to the rectal prominence above noted. (*Vide* Fig. 1.) Near the anterior end of this mass I found a small calculus projection piercing the mucous membrane; round this could be felt a dense ring of vesical tissue, on which the finger nail could make no impression. I endeavoured, with a blunt elevator, to detach this ring, but this also failed. At this juncture the spicula (Fig. 3 B) broke off at its base, leaving us, as a guide to the stone, a circular opening about an eighth of an inch in diameter. Through this I again failed to pass the director, and, as the depth of the calculus from the surface rendered manipulation difficult,

Surgeon-Major O'Farrell (who kindly assisted me at the operation) gave me valuable aid by raising and supporting the base of the bladder, as shown at Fig. 1 B. This digital support enabled me to grasp the edge of the opening with a catch forceps. Sir W. Stokes

Fig. 1



A Free calculus.
B Fingers raising encysted calculus towards abdominal wound.
C Hernia knife cutting wall of cyst.

took charge of this forceps, and I passed a hernia knife, which I guided by my left index finger, through the opening and under the dense edge of the capsule, which I slit first backwards and then forwards so as to lay bare the upper surface of a large stone. Withdrawing the knife, I passed into the sac—first on one side,

then on the other—the blades of a lithotomy forceps, which I now locked, and endeavoured to remove the mass. Under pressure of the forceps the mass broke up into what proved to be five distinct and faceted calculi, one of which, coming directly in the line of pressure, yielded, as in Fig. 2 A, while another chipped off as at Fig. 2 B. The complete removal of the *débris* was rapidly done, the sac being thoroughly freed of a calculous lining which here and there adhered firmly. After washing out the bladder with boracic solution I applied deep catgut and superficial silk sutures at the upper angle of the wound; at the lower angle I closed the cellular interval by suturing the bladder to the lips of the abdominal wound, and passed one deep suture across the lower angle. A large suprapubic drainage tube was laid in, as the patient, owing to prostatic irritability, could not endure a catheter, and antiseptic dressings were applied.

Fig. 2 A

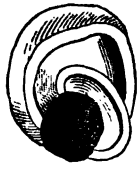
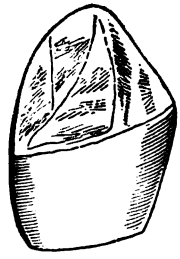


Fig. 2 B

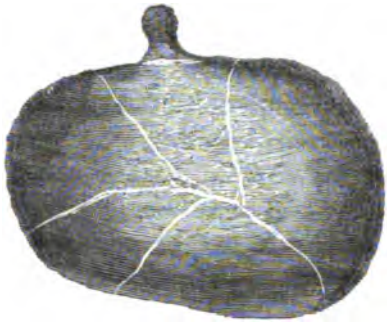


Course and Treatment.—I need not detail at length the course of this case, which (considering the condition of the patient prior to operation, and having regard to the gravity of the operation) was, on the whole, favourable until July 26th (thirteen days after operation), when, at 10 p.m., the note made is—"Patient unconscious; pulse irregular, intermittent, and extremely quick; breathing stertorous; cannot swallow; ashen appearance of face; pupils widely dilated and insensible to light; skin cold and clammy." Up to this, irrigation of the bladder had been carried out regularly with warm boracic solution, and the muco-pus, which had been copious in the beginning, had almost disappeared, and the urine itself had become fairly normal. At this stage, however, the amount lessened, and it became dark and ammoniacal. Finding him in this condition, I ordered a large enema of soap and water, a saline purge, which was followed by large doses of citrate of ammonia, and counter-

irritation over the loins and nape of neck, first with mustard, which seemed to have no effect, and later with chloroform, the application of which soon restored consciousness.

Fig. 3 A

Fig. 3 B



From this date onwards there was a gradual return of strength. In the anxious period which intervened between the operation and the time when recovery became assured, I received valuable aid from my friend and colleague, Dr. M'Hugh. Mr. Tobin, whose absence at the time of operation I very much regretted, frequently saw the case with me, and I availed myself of many of his suggestions. The accompanying Fig. 3 A shows the free calculus, and 3 B the encysted one, as broken by the forceps.

In reviewing the notes of this case I cannot but think that, had the patient succumbed on that 26th of July, death would have been recorded as the result of exhaustion. In all the cases in which death occurred after the third or fourth day, with symptoms of collapse, and where a *post mortem* was allowed, definite pathological conditions sufficient to account for the result were discovered—intestinal nephritis, renal abscess, sloughing of rectal wall, or septic infection from an unhealthy wound. Such terms as shock, exhaustion, and collapse, have no surgical significance; they are terms applied to the cause, whereas they are but erratic names for a more or less indefinite array of symptoms, and the sooner they are relegated to their proper position the better for surgical science.

NEW PROCEDURES.

Before entering on a discussion of some points of interest in the *technique* of the operation, I would briefly allude (not with the intention, indeed, of advocating them, but in the spirit of a dissentient) to recent so-called improvements on the method of Guyon. Langenbuch's sub-pubic operation (*Centralblatt für Chir.*, 27, 1888) is not likely to become fashionable, when we recollect that to reach the bladder one must dissect through the cellulo-vascular tissue between the pubes and the penis before the stone is reached, and then a hole must be made for drainage at the side of the penis.

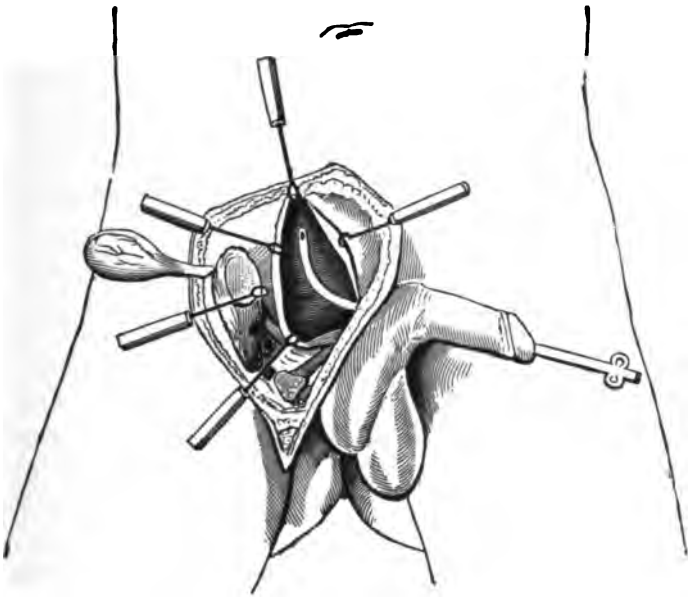
The wanton invasion of the vesico-pubic cellular tissue which this procedure necessitates would alone condemn it; but when we consider the narrow limits of the field of operation, one marvels that such a method should engage the serious attention of any surgeon of experience, and to advocate it would imply an extreme disregard for human life.

Koch (*Cent. für Chir.*, 37, 1888; *Berl. klin. Woch.*, 20, 1888) describes an operation for opening through the symphysis.

On a par with this is a recommendation from Niehans (*Cent. für Chir.*, 29, 1888), that the pubic bones should be temporarily resected to allow free entrance to the bladder. No epithet would be too strong to apply to a procedure fraught with so many dangers as this. Denunciation, however strong, would not be unjust in the face of a danger arising from the desire to be singular in the method of mutilation of our fellow-beings. The genius of invention is all very well if it confines itself to the imagination of heroic operations such as that shown in Fig. 4, taken from the *Centralblatt für Chirurgie.* When for their advocacy it becomes necessary to needlessly maim the unoffending lower animals, one can but stand by and blush for the frailties of man; but when man himself is to be made the subject of experiment, then it is time for surgeons to remember that insanity in surgery must be treated as elsewhere, and no more heed should be taken of recommendations such as these than of the vapourings of our every-day lunatic. This is strong language, but justified, when one grasps the full meaning of

tearing away the front wall of the pelvis and groping through the cellular tissue thereof when the bladder is so accessible elsewhere. I cannot imagine a surer way of producing pelvic cellulitis, urinary infiltration, and all the ills that attend the most clumsily conducted lithotomy.

Fig. 4



Very different in its inception, and indeed in all its bearing, is the method carried to perfection by Rydygier (*Wien. med. Woch.*, April 14 and 21). This is the intra-peritoneal method. With the object of utilising the plastic properties of the peritoneum, Rydygier has recommended the section of the bladder to be made through its serous coat.

This allows sufficient room for the removal of the largest calculi, and secures much more rapid healing of the vesical wound than any other method. It requires, however, for its successful accomplishment, perfect aseptic arrangements at the operation, and the bladder must not be in a state of disease likely to cause infection of the peritoneum.

If the bladder be irritable, the proper drainage cannot be carried out through the urethra; and if there be suppurative cystitis present, a thorough irrigation, so necessary in such cases, is out of the question in intra-peritoneal section.

For cases in which, from impassable stricture, the bladder cannot be distended, or when rectal dilatation is not possible, this operation may prove useful; but, with its many dangers, it cannot be accepted as a substitute for Guyon's method.

RECTAL DISTENSION.

Some time ago I listened, with interest and attention, to a communication made to this Section; and the subject of rectal distension being incidentally mentioned, an important question was asked, but remained unanswered, and that was—How much fluid should be used in the dilatation?

The experimental researches of Strong (*Annals of Surgery*, 1888, p. 22) show that 10 to 12 oz. for the rectum and 8 to 10 for the bladder render all the assistance necessary for the rapid and easy conduction of the operation. In reference to the question of rectal distension it may be well to recall the experiences of some of our contemporaries. Nicaise (*Bul. et Mém. Soc. de Chir.*, Paris, 1888, 676) details a case in which the balloon was distended with 340 grammes of fluid, and a tear of the rectum, exposing the posterior wall of the bladder, caused death on the sixteenth day. Routier mentions a case in which sloughing of a piece of the anterior wall of the rectum the size of a five-shilling-piece caused death on the twelfth day. A. Després called attention to two cases in which the peritoneum was ruptured; in each case distension with 450 grammes caused tearing of anterior rectal wall. Bouilly (*Taille Hypogastrique*, Paris, 1883) mentions a case in which rupture of the rectum took place while a preparatory distension was being carried out in a child.

These cases teach us that we should be slow to attribute death to shock, as in very many cases of supposed death from collapse the *post mortem* disclosed an interstitial nephritis or general pyæmic infection, and this latter in cases in which, in the interval between

operation and death, the ordinary symptoms of this affection were absent.

It is only now, when facts like the foregoing are brought to light, that the dangers attendant on hyperdistension of the rectum become apparent, and call for a more careful investigation of the question: How much can we inject with safety? There is manifestly some room for doubt as to the wisdom of injecting from four to six hundred grammes of fluid into a Petersen's balloon, especially when such men as Després (*Bul. et Mém. Soc. de Chir.*, Paris) make use of the following expressions in reference to it:—"There is, therefore, no necessity for the balloon of Petersen, which, to my mind (excuse the expression), is an asinine guide with which all true surgeons should be able to dispense." The same surgeon, who is also an enemy of antiseptics, and who avoids injecting the bladder, says later of Petersen's balloon—"Rest assured, it will before long go to join in the museum the old instruments of our forefathers that presently nobody will hear of."

On the other hand, Segon (*Bul. et Mem. Soc. Chir.*, Paris, 1888), says:—"To Petersen's balloon, to the facility and security it gives, is due the present position of suprapubic lithotomy." Some writers say that 600 grms. of fluid can be injected with safety. Le Dentu says that injury occurs where—1st, bowel is congenitally narrowed; 2nd, in cases of diseased walls; 3rd, when walls are weak. He, therefore, limits the fluid to 300 grms., and remarks that the bladder bears the over-distension better than the rectum.

A very important point in reference to this part of the subject has been demonstrated by Strong—viz., that injection of the rectal bag must be made before distension of the bladder, as otherwise the necessary elevation of the peritoneum will not take place. There can be no doubt that the support afforded by a rectal bag renders the operation easier of accomplishment, the only inconvenience being the great engorgement of the suprapubic venous plexus, injury to which has led to copious hæmorrhage and subsequent delay in healing. Once the bladder is reached, slight evacuation relieves the engorgement, and the veins can then be kept out of the way by retractors. It frequently becomes necessary to partially or

wholly empty the balloon after the bladder is freely opened, as intravesical manipulation is greatly interfered with by its presence. When calculi at the trigone or prostate are to be dealt with, digital pressure (as shown in Fig. 1), as used in my last case, will become necessary, and then removal of the bag is advisable. I cannot conclude my reference to this part of the subject without noting that Assendelft (whose personal experience in suprapubic lithotomy is extremely large) rarely used Petersen's balloon, and never wounded the peritoneum.

DISTENSION OF THE BLADDER.

All surgeons are agreed that fluid in the bladder facilitates the operation. Even Després, who objects to the injection of fluid, insists on the urine being retained. There is in the distension of the bladder a danger to be guarded against, and the guide suggested by Sir H. Thompson is untrustworthy. He remarks that the sensation of resistance should guide the surgeon as to the amount to be injected. It is possible that an exquisite sense of touch might be a safe guide; but most surgeons can better appreciate measured volumes of fluid than the amount of resistance offered to its injection by a thin-walled viscus, and many will agree with M. Guyon, when he says that the amount should not exceed 320 to 350 grammes, and that this is sufficient in the adult. M. Guyon has, like most of us, depended on the sensation of the resistance offered by the bladder to the ingoing fluid; but since he ruptured a bladder, as he says, "without making any effort," he takes care to adopt the safer and more scientific method of using measured quantities of fluid. Kremer (*Inaug. Dissert. Erlangen*, 1887) recommends distension by gradually raising an irrigator above the patient. In some cases of contracted and hypertrophied, or even irritable bladder, the amount above-mentioned cannot be introduced with safety, and in many cases it becomes necessary to prepare the bladder by gradual distension.

SUTURE.

The question of suture of the bladder is one which is at the present moment engaging the attention of most surgeons interested in the advance of this department of surgery. There is a strange diversity of opinion even yet as to whether the bladder should or should not be sutured, and when we study the writings of those who advocate vesical suture there is very marked divergence of opinion not only as to the method of suture but also as regards the materials to be used in such suture.

The opinions of a few surgeons, as given below, will indicate the present position of the question—"Suture or no Suture?"

Dorfwith.—Will never use it again, as it retards cicatrisation.

Perier.—If not dangerous, it is at least useless.

Le Dentu uses only *one* suture on each side, to unite the muscular and connective tissue to the bladder.

Belmas says—"To come back to it would be to bring surgery to its first infancy."

Such opinions as the above are very generally held. Many recommend complete suture, while some advocate partial closure of the wound, foremost amongst them being Guyon, who for a long time was a vigorous opponent of suture at all, and is still opposed to complete suture. He says that, applied after his method (given below), suture favours drainage, shortens the time, and directs the course of healing. The case which induced him to adopt suture was an instance of rupture of the bladder at the cicatrix six years after operation. On examining the wound which had not been sutured, he found that only the superficial parts had united, and that a funnel-shaped cavity existed at the upper and anterior part of the bladder, its base corresponding to the mucous coat which had never undergone repair (*Albarran. Bul. et Mém., Soc. Chir.* 285, 1888).

GUYON'S METHOD.

On each side of the wound at the middle the vesical wall is held up by a suspensory thread of carbolised silk; above and below this three or four catgut sutures are used to draw the submucous

tissues together. A second row is used in bringing together the muscular and connective tissue coats of the bladder. A third row of hair or silver wire closes the abdominal wound, with the exception of its middle portion, through which the drainage tubes are passed, and there tied by the suspensory threads. On the fourth day the tubes are removed, and healing is complete inside a fortnight.

Experimental research and clinical experience combine to prove that suture is not only useful, but absolutely necessary, if we are to avoid danger and procure a rapid and permanent healing after suprapubic lithotomy. There can be no better way of determining the value of suture than by comparing the results of the opponents with those of the advocates of suture, when it will be found that healing without suture took place from the twenty-first to the ninetieth day. After careful suturing, healing was complete from the seventh to the thirteenth. After suturing there has not been a bursting of the cicatrix; rupture has frequently followed healing without suture. The mortality does not seem to be much influenced by the presence or absence of suture, as in Assendelft's 102 cases without suture there was only 2 deaths, and these were not attributable to the operation. In Subbotic's (*Weiner med. Presse.*, 1887, 35 & 36) 5 cases, 3 sutured, 2 not sutured, there was no death. Of Alexandroff's 26 cases, 2 without suture healed on twenty-first and forty-seventh day; 16 with suture and superficial drainage, all healed about tenth day; 8 complete suture, no drainage, healed by primary union before tenth day.

Secondary Suture.—The question of secondary suture I may mention before I call attention to the different methods of applying the suture. Neuber (*Centralblatt für Chir.*, 24, p. 57, 1888), to avoid infiltration, recommends the following procedure:—First, the bladder is exposed by vertical incision 3 inches in length, the wound is plugged, and on the sixth or seventh day, when the cellular spaces are closed, the bladder is incised, and the stone removed. The bladder is now sutured, and six days later, if the vesical suture has taken, the abdominal wall is closed. Six operations thus conducted ended satisfactorily. Guiard (*Ann. des Mal.*

des Org. Gen.-Urin., 1887) sutures the bladder with catgut; at the same time he lays in, but does not tie, silver sutures of the abdominal wall. A double drainage tube, oval on section, is placed in the middle of wound. On the third or fourth day this tube is removed, and the wound closed by the suture above mentioned. He claims for this method the advantage that, owing to the complete drainage, the lips of the wound are kept at rest, and primary healing is allowed. The open condition of the skin incision allows free escape of urine, should any of the vesical sutures yield during the first few days; thus the danger of infiltration is avoided. By the time the drainage tube is removed the vesical wound is closed in the greater part of its extent; the tying of the silver suture then supports the line of healing.

Complete and immediate suture has numerous advocates, and in uncomplicated cases its many advantages cannot be questioned. In children especially it is advisable, since in them—first, repair is more rapid and permanent; second, the bladder is usually healthy; third, the operative procedure is easier; fourth, the lips of the incision are thick; and fifth, the position of the peritoneum is very favourable for the high operation.

MATERIALS USED.

Catgut, silk hair, and silver wire all have their advocates. Catgut sutures have a tendency to cut through vesical tissue, and in Krabbel's 12 cases there were 7 failures, the cause being the swelling of the catgut, owing to absorption of the fluids in the wound, strangulating the friable muscular fibres engaged in the suture, their vitality being thus rapidly compromised.

Hair sutures do not cut through muscular tissue (Bouilly). On the fifth day in experimental suture catgut was found loose; hair suture was firm until union was complete. Absorption of the fibres immediately in relation with the suture may take place, but as the hair does not swell, the greater portion of the tissue taken up by the suture persists. Hair suture, when kept in a 1 to 20 solution of carbolic acid for some months, becomes very pliable.

Silk is the material relied on by most surgeons now for vesical

as for intestinal suture, as much of the catgut now procurable is of questionable origin, while its condition as regard asepsis is equally doubtful. Silk is pliable, is not brittle, does not swell as much as catgut, and it can, by boiling, be made perfectly aseptic. I doubt if there is any real objection to it, and certainly for all the deeper sutures it is reliable, while Veincent, Baudon, and others, recommend the metallic suture for the superficial structures. Properly applied metallic suture affords support to the tissues round the wound, and rarely causes any destruction of tissue.

For any suture to be successful, as pointed out by Bouilly and Veincent—first, the points of suture must be close to each other; second, the threads must pass as near as possible to the mucous membrane without penetrating it; third, the orifices of the suture must be 8 to 10 millimètres apart.

In the experiments conducted on dogs by Mayimon, death occurred—first, when the mucous membrane was caught up in the suture; and second, when the sutures were far apart, allowing urinary infiltration.

VARIETIES OF SUTURE.

As in intestinal suture, so in the vesical, endless so-called improvements are made in the methods of its application. I shall briefly refer to a few of the procedures worthy of notice—

First, and probably of most importance, is the method of suture known as the Lembert, so much used in the closure of intestinal wounds. This suture is shown in Fig. 5. If this suture be applied at regular and short intervals, the bladder can be hermetically sealed, broad surfaces of vesical tissue are brought into contact, and where the bladder is sound rapid and firm union takes place.

Second.—Rydygier (*Wien med. Woch.*, April 14 and 21, 1888) uses a double suture alternating as in Fig. 6. This, but for the delay occasioned by the laying-on of the primary or finer suture, is the most reliable of the varieties of suture. Veincent also advocates this suture, recommending catgut for the deep, metallic for the superficial line. All the threads being cut off close to knot, the catgut sutures are absorbed, the metallic become encysted.

Fig. 5

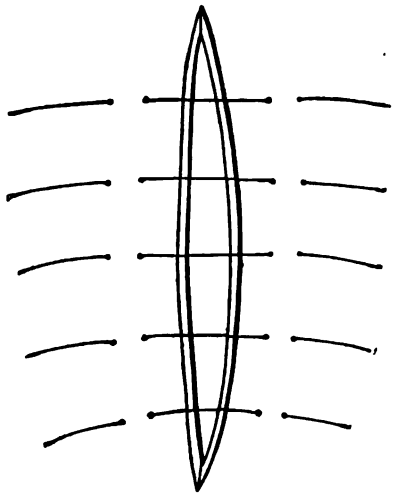
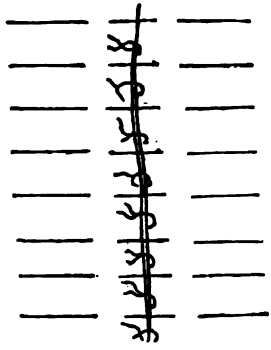


Fig. 6

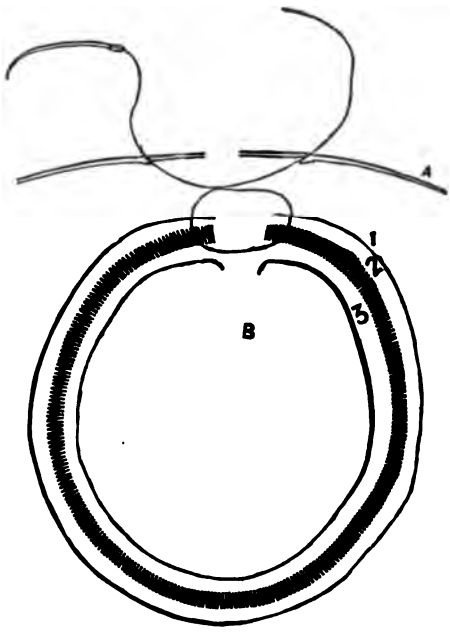


Third.—Gehler (*Taille Hypogastrique*, Bouilly, Paris, 1883) used a suture taking up the walls of the bladder and the abdominal wall. This simple *vesico-cutaneous* suture is an unsafe one, since the abdominal wall may heal rapidly, and the bladder wound remaining open, infiltration is almost certain to occur.

Fourth.—Baudon (*Taille Hypogastrique*, Bouilly, Paris, 1883) describes a crossed vesico-cutaneous suture (Fig. 7), recommending

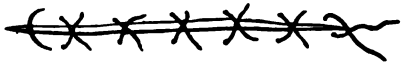
metallic threads. This is open to the same objection as the preceding one, and is not likely to find many advocates.

Fig. 7



A Abdominal wall. 1, Fibrous; 2, Muscular; 3, Mucous Coat of Bladder.

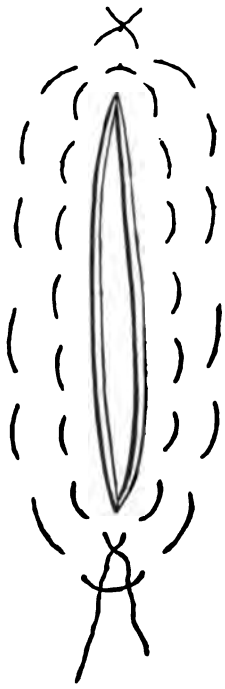
Fig. 8



Fifth.—Simple continuous suture (Fig. 8) has been employed, but as all sutures are liable to cut through the vesical tissue, if any part of a continuous suture presses unduly, strangulation and breaking down of the tissue takes place, and the entire line of suture becomes ineffective. Few now depend on a continuous suture for vesical or intestinal wounds.

Sixth.—Some surgeons use a deep interrupted suture, and a superficial continuous suture. This is an easily applied and fairly effective suture. I have used it in longitudinal wounds of the intestine successfully.

Fig. 9

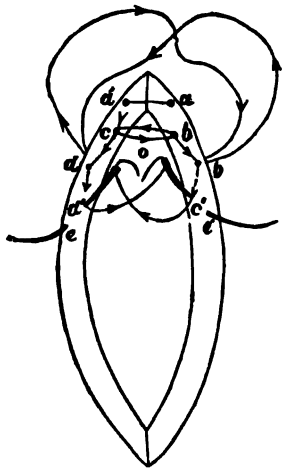


Seventh.—Brenner (*Arch. für klin. Chir.*, Bd. 35, H. 1) describes a variety of suture aptly called "Purse-string" (Schnürnaht). The wound is surrounded by two threads (Fig. 9), one in the submucous tissue, close to the edge of the wound—a second in the muscular tissue, 8 millimètres external to this. Traction on these threads puckers up the tissues of the bladder into a solid mass at the site of the wound. In his experiments Brenner found that hyper-distension caused rupture of the bladder, but never at the line of suture. He experimented on the dead subject, then on

dogs, and finding the method reliable, applied it in his cases with complete success. I have applied this suture with satisfactory result where I found the bladder-wall thin, but where the vesical tissues are normal Lembert's method is more satisfactory, as it does not endanger the vitality of a large piece of the bladder-wall, such as would occur if either suture in Brenner's method became too tight through swelling of the tissues or the threads.

Eighth.—Another continuous suture (Fig. 10), which may be called lace suture, is the one described by Neidzwiedzki (*Centralblatt fur Chir.*, 1889, 30). I have no experience of this method, but I should say it is difficult of accurate application, and of course it is open to the same objection as all continuous sutures.

Fig 10



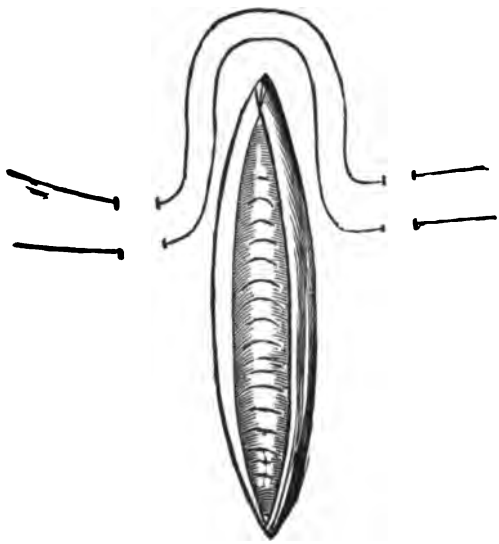
The "Suture Préalable" (Fig. 11) is recommended by Duchastelet, and for its application the instrument (Figs. 12 and 13), taken for Bouilly's work (*Taille Hypogastrique*, Paris, 1883), affords an easy and accurate way of applying it. The advantages claimed for this previous suture are—

1st. It permits the symmetric and regular placing of the threads while the bladder is still full, and easily got at by sight and touch, and neither blood nor urine obstructs the field of operation.

2nd. The instrument serves as a guide for the length of the vesical incision and direction of the bistoury.

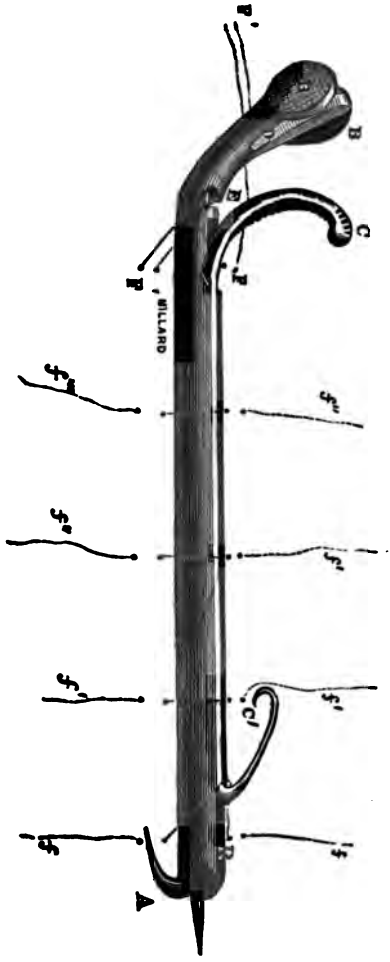
3rd. After removal of the stone the vesical surfaces can be rapidly brought back to back, hermetically sealing the wound.

Fig. 11



Notwithstanding the diversity of opinion as to the extent of suture, the materials to be used, or the method of application to be adopted, most surgeons agree that to leave the wound open, as suggested and practised by Thompson, is to inordinately delay healing, and to expose the patient to many accidents avoidable by careful suture. And I think I am expressing the opinion of the majority when I say that complete suture should be carried out in children always, and in adults when the bladder is healthy. When the bladder is diseased the suture and drainage described by Guyon becomes necessary.

Fig. 12



In reference to this subject in children, Alexandroff, after an exceptional experience, concludes that—

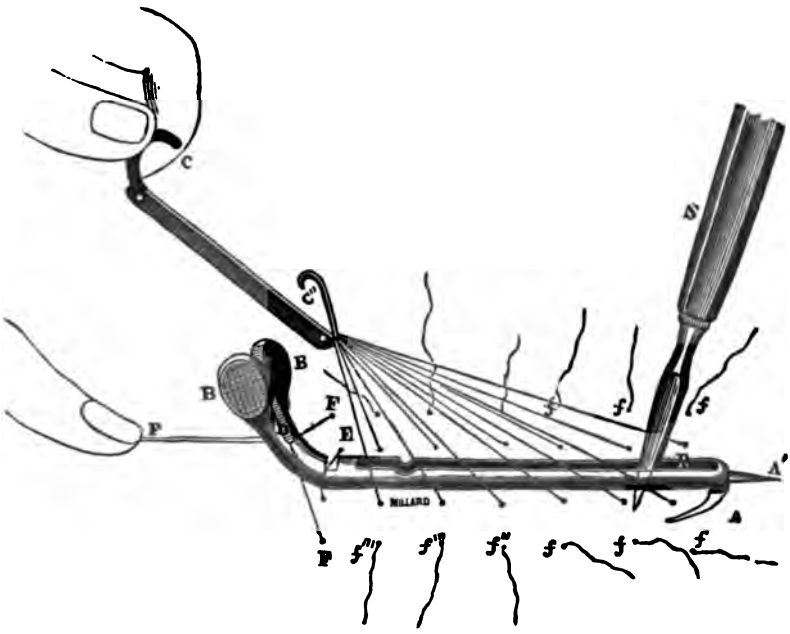
1. High section is best in children.
2. Vesical wall should always be sutured.
3. Silk is the best material.

4. Drainage of the pre-vesical tissue is unnecessary.

5. The catheter may be removed on the third day.

6. An alkaline condition of the urine is no bar to the operation or to suture.

Fig. 13



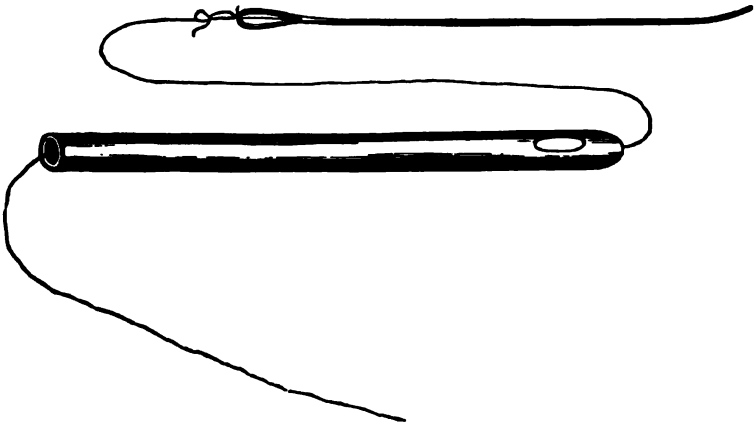
DRAINAGE.

In many cases where the bladder is healthy, and complete suture has been adopted, a catheter allowed to remain in the bladder for a few days is, with regular catheterisation afterwards, sufficient, but where cystitis is present it becomes necessary not only to drain, but to irrigate the bladder for more or less lengthened periods.

For such cases Keyes ("Annals of Surgery," 1888) recommends a small opening to be made in the perinæum, through which a No. 30 (French elastic) catheter is drawn by means of a probe, to which is attached a strong silk thread, which has been passed

through the point of the catheter, as in Fig. 14, a knot being left within the catheter.

Fig. 14



The most usual method of draining and irrigating is by Perier's double suprapubic tube. This is the appliance which Guyon advocates for all cases. The curve which is necessary to make this appliance fit easily over the pubes is a permanent one. Ordinary elastic tubes are very unmanageable, but the double tube, oval on section, described by Guiard, answers admirably when drainage and irrigation are indicated.

Heretofore it was customary to lay the tubes in the lower angle of the abdominal wound; but most surgeons are now agreed that it is best to close the vesico-pubic cellular interval by sutures carefully applied and drain through the middle of the wound (Englebach, Rollin, *Annales des Mal. des Organes Gen.-Urin.*, 1887).

STATISTICS.

I have avoided introducing the statistics of this operation; for, without an exhaustive analysis of numbers of cases (especially those instances where individuals record large personal experience of the procedure), it is absolutely useless to lay down rules as to the cases in which this operation should or should not be done. This will

become apparent when one endeavours to make a communication more than a mere synthetical corner-stone on which to raise a monument in advocacy of the revived operation. When we recollect the unfortunate but natural tendency to publish successful cases, and to withhold fatal ones, we must recognise the difficulty of determining from figures alone the real position of this operation.

Nevertheless, a careful study of the course and history of the cases operated on during the last decennium, a full and unbiassed hearing of the two sides of the question, as debated in the councils of the surgical world, will go far to place on a firm basis an operation which asepsis in surgery has raised from the limbo to which the less elegant and less trusty methods of our forefathers consigned it.

ON A SERIES OF ONE HUNDRED CATARACT EXTRACTIONS.

By H. R. SWANZY, M.B., F.R.C.S.;
Surgeon to the National Eye and Ear Infirmary; Ophthalmic Surgeon to the Adelaide Hospital.

[Read in the Section of Surgery, February 21, 1890.]

Seventeen years ago, a communication on "Accidents in Flap Extraction of Cataract" was made to the old Surgical Society of Ireland by Dr. Archibald Jacob; and, since then, the subject of cataract extraction has not been brought forward in this place. Yet it seems desirable, that an operation, surrounded, as this one is, with so much interest from a humanitarian, as well as from a purely surgical, point of view, should occasionally form the subject of a communication to this Section of the Academy, although it is only some of those here who practise ophthalmic surgery.

Eventful as these seventeen years have been in the progress of general surgery, they have not been less so in the progress of ophthalmic surgery; and in nothing is this more apparent than in the statistics of the operation for cataract. As the chief agents in these improved statistics we recognise, above all, those antiseptic measures which have played so remarkable a part in other branches of operative surgery, and, next to these, the discovery of eserine and of cocaine.

From time to time, during this period, operators have proposed and practised new plans for different steps of the operation, and have devised new instruments for carrying out these new plans. But, although I may incidentally refer to one or two of these, I do not intend on this occasion to discuss any of them at length. Some of these proposals have been very generally adopted, while, again, others of them have had but an ephemeral existence.

Just at the present time, the great question which is paramount in the minds of ophthalmic surgeons, as regards the operation of cataract extraction, is—Whether an iridectomy should form part of the proceeding, or not? Into this discussion a good deal of warmth has sometimes been introduced, and we hear some of those who do not practise iridectomy describing that step as "mutilation of the iris," although they have not yet got so far as to call the iridectomists "mutilators." And, on the other hand, an iridectomist, who holds a high position in our profession, has stated it to be his opinion that it is not moral to leave out the iridectomy. All this is to be regretted, for it must be taken for granted that each one of us, whatever method he employs, is animated by the desire to do that which seems to him the best for his patients, and for the art he practises; and difference of opinion here, as elsewhere in medical science, must rather be useful for the general welfare than the reverse, if only the discussion of the point at issue be approached in a spirit of tolerance.

The operation which I perform is commonly known as Von Graefe's, although it differs in some important particulars from the method as Von Graefe left it to us. It is also known as the three millimètre flap operation. I operate as follows:—About half an hour before the operation a drop of eserine solution (4 grs. ad. ℥i.) is put into the eye to contract the pupil; and the patient's face, especially the eyelids and their neighbourhood, is carefully washed with hot water and soap. The instruments are washed with absolute alcohol, then boiled in water, and then laid ready for use in a bath of a 1 per cent. solution of carbolic acid. After the patient has been placed upon the couch, the eyelids are everted, and the conjunctival sac is thoroughly washed and wiped out with lint soaked in a 1 in 5,000 solution of corrosive sublimate. The same sublimate solution is used all through the operation for irrigating the wound and surface of the eyeball, and for washing away blood coagula and morsels of cortical substance, as may be required. No sponges are used, but, in their place, small bits of lint, which have been boiled in sublimate lotion, and which are kept stored for use in a closely-covered glass jar filled with sublimate lotion. The patient receives about three drops of a 2 per cent. solution of

cocaine in the eye, with an interval of about five minutes between each drop, before the operation begins. This solution, as also the atropine and eserine solutions, is not made with plain water, but with a 1 in 5,000 solution of corrosive sublimate.

A wire lid-elevator having been applied, the eye is fixed with a forceps by a fold of conjunctiva and sub-conjunctival tissue below the vertical meridian of the cornea. The point of a Von Graefe's knife is entered just in the margin of the clear cornea, at the outer extremity of a horizontal line, the centre of which would lie 3 mm. below the summit of the cornea. The point of the instrument is then passed cautiously through the anterior chamber, and the counter puncture is made in the corneal margin, at the inner extremity of the imaginary horizontal line, just described, and then the incision is finished in the upper corneal margin by one or two slow strokes of the knife. While this is being done the aqueous humour flows away; but, owing to the action of the eserine, and to the slow way in which the aqueous has been allowed to flow off, the iris does not prolapse.

The fixation of the eye is now given over to an assistant, and the surgeon, passing a curved iris-forceps, or, perhaps better, a Liebreich's iris-forceps, into the anterior chamber, seizes the smallest possible portion of the sphincter of the iris, at a point corresponding to the centre of the corneal incision, draws it out, and, with the forceps-scissors, cuts out a very narrow bit of iris. I like to produce a coloboma of 2 mm. (Fig. 2) to 3 mm., and not more than 4 mm. in width.

The operator now takes the fixation-forceps from his assistant, who gently raises the speculum with the eyelids off the globe, so that no pressure may be made on it during the remainder of the operation. The cystotome is then introduced, and the anterior capsule divided freely. Until recently I scored the capsule chiefly from below upwards several times, making one final horizontal division of it along the upper margin of the lens. But I have of late practised horizontal division of the capsule exclusively, making several transverse scorings at different levels of the anterior surface of the lens. My reason for this is, that, while an equally

free opening is made in this way, I think the cystotome, as it is being withdrawn, may be less likely to drag tags of the capsule into the corneal wound—an event which it is very desirable to avoid—than when the instrument is drawn directly towards the wound in opening the capsule. But I must state, that I know the method does not form a guarantee against a tag of capsule being found in the wound. I have occasionally employed the capsule-forceps for tearing a bit out of the centre of the capsule, as recommended by some surgeons. I have never used what is known as the peripheral capsulotomy. Both of these methods have much to recommend them; but I think there are disadvantages connected with each, which more than neutralise its advantages.

The cataract is now delivered by placing the edge of the hard rubber, or tortoise-shell, spoon just below the lower edge of the cornea, and making with it gentle pressure backwards, until the upper margin of the lens presents in the wound; then, the same pressure being maintained, the spoon is advanced over the cornea, pushing the lens before it and out through the wound; but, as soon as the greatest diameter of the lens has passed the lips of the wound, the pressure of the spoon must be instantly diminished, lest rupture of the zonula, with prolapse of the vitreous, be caused. The fixation forceps and speculum are now removed from the eye, and a cold compress, with sublimate lotion, is laid on the closed eyelids for a few minutes.

The condition of the pupil is then examined, and, as a rule, it is seen to be clear and black. On the other hand, it may be found that some of the cortical portion of the cataractous lens has not come away. It is well to clear the pupil of this, so far as possible, although very slight remains have no serious significance. For this purpose I employ, exclusively, what we know as "the lid manœuvre." Facing the patient, the operator raises the upper lid with the thumb of his left hand, while he lays the tips of the first and second fingers of his right hand on the lower lid, and, with the latter, makes slight rotatory motions over the cornea, so as to collect the masses towards the pupil, and then a few rapid light motions upwards with the margin of the lid will usually drive these

masses towards and out of the wound. But great care and delicacy of touch are called for here, as any undue pressure on the eye may rupture the hyaloid. Some operators stand behind the patient, and make the lid-manœuvre with the thumb, which does not seem to me to be a good method, for the delicacy of touch with the thumb is inferior to that with the first and second fingers.

I do not employ irrigation of the anterior chamber, or intra-capsular injection, for the purpose of washing out cortical masses, as recommended by M'Keown, Wieckerkiewicz, and others. I rarely operate on unripe cataracts, and, consequently, I find no use for this proceeding as a routine method; while in those exceptional cases in which considerable cortical masses remain, and where they cannot all be delivered by the lid-manœuvre, I prefer to leave some of them behind, to be dealt with later on, if necessary, by discission, rather than apply irrigation, which seems to me a somewhat rough measure. I did use Dr. M'Keown's syringe on one occasion in this series to wash out cortical remains, without effecting the desired end. Perhaps I should have employed more force, but I shrank from doing so. Subsequently, I succeeded in expelling the masses by the lid-manœuvre in the ordinary way, and I think it likely I was assisted in this by the fluid which had been introduced into the anterior chamber by the syringe. Both before and since this experience, I have often thought that assistance in the expulsion of cortical masses might be obtained by simply introducing a few drops of suitable fluid into the anterior chamber; but I have not as yet carried out the idea in practice. We know that cortical masses often come away more readily, after we have waited for the formation of some aqueous humour.

The next step is one which I think is peculiar to myself, at least I have not seen it described by anyone else; nor, of the many oculists I have seen operate did any practise it. It consists in searching the corneal wound for any tag of capsule, which may have become prolapsed into it. Owing to the transparency of the capsule, a tag of it may lie in the wound but be invisible to the surgeon, and, if allowed to remain, it becomes incarcerated in the cicatrix, and can, even long afterwards, give rise to serious and

deep-seated mischief. This mischief may take the form of chronic irido-cylitis, or it may appear in the form of acute purulent inflammation of the uveal tract originating in the cicatrix.[a] Again, Mr. Treacher Collins has recently[b] pointed out, that another serious condition may arise from entanglement of capsule in the cicatrix, for his investigations have shown it to be extremely probable that the accident may sometimes produce a tendency to glaucoma. It is my routine practice, then, at this stage of the operation, to pass the points of a curved iris-forceps, slightly opened, between the lips of the wound, then to close them, and to cautiously draw them back again. Frequently, it will be found that a tag of capsule has been captured by the forceps, and, if so, it is snipped off with the scissors; or, it may be, that no capsule is caught. The forceps is then similarly inserted at an adjacent part of the wound, and in this manner the wound is searched from end to end for capsule. In the one hundred operations of the series, a tag of capsule was found in the wound nine times. This proceeding is an extremely delicate one, and requires the greatest caution on the part of the operator. I have not myself had any disaster during its performance, but, as it must be done without fixation, the surgeon has to be on his guard against an unexpected upward motion of the patient's eye, which would bring the points of the forceps into the vitreous humour; and, when he has captured a tag of capsule, he must draw it out sufficiently, and yet not too far, or he will rupture the hyaloid. Indeed, one sometimes comes across patients who have such restless eyes, that it is quite impossible to search for capsule in the wound. This has happened to me three times. I think that this proceeding is an important one, and that it almost removes the one serious drawback to this method of cataract extraction.

The last point in the operation is to see that the whole of the iris has returned to the anterior chamber; and, in order to be quite

[a] Dr. A. Wagenmann, in his recent important paper, Ueber die von Operationsnarben und vernarbten Irisvorfällen ausgehende Glaskörpereiterung, A. von Graefe's Archiv, XXXV. 4, brings microscopic proof to bear upon this and upon other interesting points.

[b] Trans. Ophthal. Soc. January 30th, 1890.

sure of this, it is necessary to pass a narrow, and slightly curved, silver spatula into the anterior chamber, and to stroke down each pillar of the coloboma, as far as it can be brought. The influence of the eserine, dropped in before the commencement of the operation, being still present, the iris responds to this manipulation all the more readily.

The surface of the eyeball, and especially the wound, having been irrigated with the sublimate lotion, a drop of solution of atropine is instilled into the eye, and the dressing is applied. This consists in a piece of lint, which has been boiled in sublimate lotion, and of a size sufficient to extend ¼ inch beyond the orbital margin in every direction; again soaked in sublimate lotion, and laid on the closed eyelids. Pledgets of absorbent cotton wool, also previously boiled in sublimate lotion, and now again soaked in it, are placed over this piece of lint in such a way that the bandage, when applied, may exert equal pressure on every part of the eyeball. Over this again comes a layer of oiled-silk protective; and then the bandage—a narrow roller of fine flannel, which passes three times over the dressing and round the head.

The patient remains in bed for two or three days. The dressing is changed for the first time after 48 hours, and then once every 24 hours for a week, when it is left off. The eye should be protected from strong light and from cold until it is quite white, but glasses can usually be ordered in a fortnight, or three weeks.

And now with regard to the results which I have obtained by this method in 100 consecutive cases—the vision noted being that possessed by the patient when last seen, and the last of the series being the final extraction in the year 1888[a]—there were only two failures; and these were the result of suppuration, and were the only cases in which suppuration supervened on the operation.

There was one case in which the resulting vision was finger-counting at 2·5 m. This was one of three cases, in which a considerable amount of cortical substance remained behind. The

[a] This paper was to have been read last Session but that circumstances interfered with this being done. All the extractions performed in 1889, and so far in 1890, have been successful.

patient was discharged twelve days after the operation with the "eye quiet and doing well," and he was to come back for further treatment, which would probably give him very much better sight, but he has not returned.

There were two cases in which vision amounted to finger-counting at 4·0 m. Of these, one had a patch of central choroiditis, which would have prevented any better vision, even if the patient had never had cataract. In the second case, a minute bead of vitreous presented in the wound, just as the patient was closing his eye after the speculum was removed, became incarcerated in the cicatrix, and, no doubt, gave rise to some irregular astigmatism with its consequent deterioration of vision.

There were two cases in which the vision was finger-counting at 6·0 m. In one of these there was a thick capsule, which the patient would not remain in hospital to have torn, or else his vision might have been vastly better, but still he was enabled to read large print (with + 16·0). In the other case, too, there was a thick capsule, which was torn, but unfortunately the house surgeon failed to take a note of the improvement that resulted, so that I am unable to credit my statistics with it.

In one case the vision was 6/60. And here there was tobacco amblyopia.

In twelve cases the vision was 6/36.

In eight cases the vision was 6/24.

In thirty-seven cases the vision was 6/18.

In seventeen cases the vision was 6/12.

In fifteen cases the vision was 6/9. Of these one became glaucomatous some months later.

And, in three cases, the vision was 6/6.

Briefly stated, according to a conventional method, there was good vision in 93 per cent., moderate vision in 5 per cent.,[a] and no improvement in 2 per cent.

[a] Of these one case probably did obtain good vision, but no note was taken of the final result; a second case had central choroiditis, which alone interfered with what would have been good vision; while two cases might have obtained good vision had they submitted to a slight secondary operation.

If the advanced age of most cataract patients be alone taken into account, the restoration of what we call normal vision can be but rarely expected; but, with a vision of 6/36, and even less, patients operated on for cataract can get about freely, and can even enjoy their sight for reading.

Now, the foregoing results, as they stand, are satisfactory. But they would present a yet more favourable appearance, were it not that they are for the most part the results noted a fortnight or three weeks after the operation. It is not often possible to keep patients in hospital for a longer period, and even many private patients, when they once obtain their spectacles, do not present themselves any more to the surgeon. But frequently, when I have had the opportunity of testing the sight some months afterwards, I have found the acuteness of vision improved in a marked degree, from what it had been soon after the operation. As an example of this, I may mention the case of a Catholic clergyman from the South of Ireland, upon whose right eye I operated in July, 1887. The vision a fortnight afterwards, just before his return home, was 6/36, but, a year later, I found the sight improved to 6/9 without a change of glass. I then operated on the left eye, and thirteen days later its vision was 6/60, but when, after eleven months, he paid me a visit, I found the sight of this eye, too, 6/9 with the same glass. I could give several other similar instances, and I have no doubt that if all the eyes in the series could have been examined some months after they were operated on, a much higher average standard of sight would have been noted.

I had not to record a falling off in the power of vision after a time in any of the eyes operated on, except in a case of glaucoma, and in those cases in which the capsule became slightly thickened. These latter were, of course, readily put right by discission, but to them I shall again refer.

Accidents during Operation.—Portions of the cortical substance were left behind eleven times. In eight of these cases the remains were insignificant in amount, and had no influence whatever upon the success of the result. But in the three other cases a considerable portion of cortical substance had to be left. In two of these

the cataract had been artificially matured, and probably was not yet sufficiently ripe at the time of the extraction. One of them (already referred to) was very capable of considerable improvement by a needle operation, but did not have it performed. (V.=Fingers at 2·5 m.) In the other case the cortical remains gave rise to some increased tension, which subsided when the masses became absorbed, and at the time of the patient's discharge V.=6/36. In the third case the cortical remains were needled, and V.=6/18 obtained.

Loss of vitreous occurred only twice in the whole series of one hundred cases. In one case there was a defective zonula with fluid vitreous, and the cataract had to be delivered with the vectis (V. obtained=Finger-counting at 4·0 m. Patch of central choroiditis). In the other case, the lens being remarkably small, I did not think I had delivered it all, and, in applying the spoon to the cornea in order to press out some hypothetical cortical masses, I caused slight loss of vitreous to take place. (V.=6/18.) In one case some vitreous presented in the wound just as the operation was concluded, and became incarcerated in the cicatrix. (V.=Finger-counting at 4·0 m.) These are the only accidents during operation which I experienced in this series.

Anomalies of the Healing Process.—There were, as already stated, two cases of suppuration of the wound. Both were in-hospital patients. In one of these the operation was perfectly normal, and I failed to trace any way in which the eye could have become infected. Yet, there is no doubt but that, in some particular, the antiseptic precautions were defective. In the other case of suppuration there was some difficulty in delivering the lens, probably from insufficient eyeball-tension, and after delivery a bead of vitreous presented in the wound, but there was no loss of it. At the end of the operation the eye looked well. Here, too, I credit the suppuration to some oversight in the antiseptic measures, rather than to the unimportant irregularity in the operation. In both of these cases the eye was lost, and, as I have said, they were the only total losses in the series.

Iritis was seen only once in the whole series. It appeared on

the third day, and was very slight in degree, the eye gaining 6/18 of vision, 18 days after the operation. The almost total absence of severe plastic iritis is eminently satisfactory, because it is a process by which formerly many an eye was lost. It is now held to be as much an infective process as is suppuration of the wound, and the freedom from it which we enjoy must be referred to the antisepsis. Of course, in a good many of the cases, there were a few fine synechiæ between the pupillary margin and the capsule, but these have no such significance as that possessed by the violent plastic iritis, which may jeopardize sight.

Incarceration of the iris in the cicatrix occurred once. In this case the patient was very unruly during the operation, so that the attempts at reposition of the pillars of the coloboma had to be abandoned. (V.=6/6.) In several cases there was an anterior synechia, an adhesion of a small portion of the iris to the posterior surface of the corneal cicatrix, but without any prolapse of the iris in the wound. In my opinion, this condition is not of importance, although there are some who do not think it innocuous. I believe myself that this anterior synechia is only liable to occur in those cases where, owing to some unknown cause, the wound in the corneal margin, instead of becoming sealed in the course of a few hours after the operation, with restoration of the anterior chamber, remains patent for a more lengthened period—it may be for several days. During this interval the iris is in contact with the cornea, and with the inner aspect of the wound; and then, between it and the wound, delicate adhesions form, which are not always broken down when, the wound healing, the anterior chamber refills, and the iris returns to its normal plane. Mr. Priestley Smith considers this anterior synechia more likely to occur when no iridectomy has been made.

In one case, already twice referred to, a small prolapsed portion of vitreous healed in the cicatrix, and caused slight bulging of the latter. Vision resulting=Fingers counted at 4·0 m.

In one case the patient was attacked with gouty scleritis nine days after the operation. Two days later he got a marked attack of gout in his toe, and this, along with the scleritis, rapidly gave

way to salicin and colchicum without any ill result to the eye. $V=\frac{6}{12}$.

A remarkable anomaly in the healing process occurred in one case. It consisted in an extraordinary long interval between the operation and the complete closure of the wound. The operation itself was absolutely normal, there being not even an atom of cortical substance left behind. There was subsequently no inflammatory reaction, either in the wound or in the iris; while the patient's health was perfectly good. Yet, although the eye was kept carefully bandaged, and other measures were taken, the wound did not completely close for a period of eight weeks, during which time the aqueous humour was dribbling away. The case was seen by several ophthalmic surgeons, who all declared they could find nothing to account for the tardiness in closing, and to this day I have not myself been able to account for it. I repeatedly searched for capsule, which might have prolapsed into the wound, but never found any. Finally, the wound did close, as stated, after eight weeks, and the patient obtained 6/9 of vision, which he enjoyed for five months. Chronic simple glaucoma then set in, and this necessitated operative treatment. The case is still under observation, but it is to be feared that marked deterioration of vision will be the consequence of the glaucomatous process. How far the delay in healing may have had to do with the subsequent glaucoma as its cause would be an interesting subject for consideration, but one upon which I cannot enter here. It is right to state that there was one small anterior synechia, at one side of the coloboma.

As regards secondary operations—in addition to sclerotomy in the case which became glaucomatous—discission of the capsule was performed in 21 of the 100 eyes of the series, and a needle operation, to break up cortical remains, was performed in one eye.

I am, then, one of those who perform an iridectomy in the course of every cataract extraction; but the coloboma which I make is a narrow one. I hold to the iridectomy because the writings of surgeons who perform the simple extraction have not, I think, shown it to be as safe a procedure as this one; for, in the

simple extraction, it is not yet possible to guard with certainty against prolapse of the iris occurring, not merely at the time of the operation, but even after it is concluded, and after the bandage has been applied. The consequence of this prolapse is incarceration of the iris in the cicatrix—a condition, as all ophthalmic surgeons know, of serious import. In such eyes, after a few weeks, the incarcerated iris is liable to become cystoid and bulging, causing displacement of the pupil and irregular curvature of the cornea, with resulting deterioration of sight. Nor is this all. Such eyes, even more frequently than those in which a tag of capsule has become incarcerated in the cicatrix, are liable, weeks, months, and even years after the operation, to take on severe irido-cyclitis either of the plastic or purulent form, ending in total loss of sight. And in these eyes I think inflammatory reaction, often of a serious character, is more apt to attend upon a slight secondary operation, such as discission of the capsule, than in those eyes where no incarceration of iris exists.

In Prof. Knapp's last published and interesting series of one hundred cases, by the simple method, incarceration of the iris occurred in two cases. But there were four other cases in which, in order to avert incarceration, the author had to abscise prolapsed iris on the 5th, 6th, 9th, and 14th day, respectively, after the operation; while in a fifth case the swollen and cystoid prolapse was abscised two months after the primary operation. And I think it will be admitted by a large proportion of ophthalmic surgeons, amongst whom I may probably reckon Prof. Knapp himself, that, when once prolapsed iris has lain in the wound for even three or four days, it is difficult, if not impossible, to be sure that any measure we can adopt will reduce it, so that no portion of it will remain incarcerated. Nevertheless, this record of Prof. Knapp's is a remarkable and an unusually good one for the simple operation, so far as my reading informs me. In the series of another friend of mine, Prof. Stedman Bull, which was operated on by the same method, the iris was incarcerated in fifteen eyes; and this, too, is a good record. But in this series of mine the iris was incarcerated once only; and in this one case the incarceration was not due to secondary

prolapse, but to the impossibility of completely reducing the pillars of the coloboma during the operation, owing to the uncontrollable restlessness of the patient.

Nor was there in any case, except in the one just referred to, a cystoid or bulging cicatrix. Nor had I, in this series, to resort to any secondary operation in order to reduce a secondary iris-prolapse, because no such prolapse occurred.

I think, too, the clearing of the pupil of cortical remains must often be a more incomplete detail, and must be attended with greater danger to the hyaloid, in the simple method than in that with iridectomy.

The advantages claimed for the simple extraction are, first of all, the round and movable pupil. Now, so far as the question of beauty is concerned, I readily admit that an eye with a circular pupil is a prettier object than one with a keyhole pupil; and when this round pupil is movable—*i.e.*, when there are no synechiæ, æsthetic perfection is attained. But, I submit that the patients with whom we have chiefly to do in these cases are advanced in life, and that to them the question of personal beauty is altogether secondary to that of useful sight; consequently, that the operation to be preferred for them is that one which will give to the greatest number of them comfortable and lasting vision.

Again, it is claimed that the simple operation gives a higher average acuteness of vision than the operation with iridectomy; and it is also stated that persons operated on by the simple method have a better power of guiding themselves—a better power of "orientation," as it is termed. But in all this discussion one point is constantly left out of view, or passed over in silence—a point, to my mind, of very great importance—namely, the position and width of the coloboma, which those surgeons, who now advocate the simple operation, were in the habit of making in connection with their operations, before they adopted the simple extraction. There are iridectomies and iridectomies, colobomata and colobomata. Operators, of whom, as I know, there are many, who thought it necessary, as at one time I myself did, to remove a portion of iris corresponding to nearly the whole length of the wound at the

corneal margin (Fig. 1), would, I can understand, be amongst the first to fly to the other extreme of making no iridectomy at all. And, no doubt, with small circular pupils they obtain a better acuteness of

Fig. 1.

vision and better orientation, than they previously did with their enormous colobomata, even if the latter were made above and partially concealed by the upper lid. But I know, moreover, that amongst those who have adopted the simple operation, there are surgeons whose practice it was to extract through a section made in the lower margin of the cornea, and with a wide iridectomy. That the simple extraction compares favourably with such a method, I shall not deny. But in the method which I employ the coloboma is very narrow—some 2 to 4 mm. wide (Fig. 2).

Fig. 2.

It is easy to understand why, in the simple extraction, prolapse of the iris with subsequent incarceration is so liable to occur, even some days after the operation, and why it is so difficult to devise a sure means for preventing the accident; as, also, how it is that even a very narrow coloboma is sufficient to protect the eye from this disaster. And yet I am inclined to think, that among those oculists who have reverted to the simple method, there are some who do not realise the *modus operandi* in either case. Within a few hours after the operation, as I have already mentioned, the wound in the corneal margin most commonly closes, the aqueous humour collects, and the anterior and posterior chambers are restored. But it takes many hours more for the delicate union of the lips of the wound to become quite consolidated, and during this time it requires but little—a cough, a sneeze, a motion of the head, the necessary efforts in the use of a urinal or bed-pan, no matter how careful

the nursing—to rupture the newly-formed union; and, as a matter of fact, this often does take place. The aqueous humour then flows away through the wound with a sudden gush, and, where the simple extraction has been employed, carries with it the iris. Doubtless, in this event, it is that portion of the aqueous humour which is situated behind the iris in the posterior chamber, which is chiefly concerned in the iris-prolapse; the aqueous humour in the anterior chamber probably flows off without influencing the position of the iris. The advocates of the simple operation endeavour to prevent secondary iris-prolapse by a spastic contraction of the pupil, produced by eserine, which is instilled at the conclusion of the operation, and, again, by some operators, a few hours afterwards. In most instances the desired end is by this means effected. But there is a considerable percentage of the cases, in which the contraction of the sphincter iridis is overcome by the pressure of the aqueous humour from behind, and iris-prolapse takes place.

How, then, does the formation of a coloboma prevent prolapse of the iris when the wound bursts, as I have described? Not because the portion of iris which is liable to prolapse has been taken away. That would mean nothing less than the whole of that part of the iris which corresponds to the length of the opening in the corneal margin. But the coloboma averts secondary iris-prolapse, because it provides a gateway for the aqueous humour, contained in the posterior chamber, to escape directly through the wound, without carrying with it the iris in its rush; and it is evident, that the narrowest coloboma which can be formed will be amply sufficient for the purpose. To my mind, a narrow iridectomy here is no "mutilation of the iris," but rather a measure which rests upon a sound scientific basis, and which is calculated to insure the safety of the eye in an important particular.

As to disfigurement of the eye, there is practically none, when the coloboma is so narrow, and is situated in the upper part of the iris. The pupil, too, is movable, almost if not quite, as much so, I venture to say, as in most cases of simple extraction. For it is entirely a mistake to suppose that a narrow coloboma renders the pupil immovable. Where there are no adhesions between the

pupillary margin and the capsule, as frequently happens, the reaction to light is active, a drop of atropine will dilate the pupil widely, and a drop of eserine will contract it.

That the average acuteness of vision, in cases operated on by the simple method, is higher than in those operated on with a small iridectomy, is a statement of which, I think, there is no proof. The average acuteness of vision in Prof. Knapp's last series is no doubt very high, and I have seen it quoted as convincing evidence of the advantage, in this respect, of the simple method. But the high acuteness of vision in that series was not due merely, if indeed at all, to the round pupil; but rather, as Prof. Knapp himself states, "the cause of this gain was the subsequent discission of the capsule, which was done in seventy-four cases." In Prof. C. Stedman Bull's recent series, discission was performed in fifty out of the one hundred cases, and his average vision is very good. Doubtless in my cases a higher average vision would have been obtained, had I discised the capsule in nearly every case, instead of in only twenty-one of them. But we must take the public as we find them; and in private practice in this country, when patients obtain sight which enables them to read and write fluently, or even, sometimes, to sketch and play billiards, as well as to go about with comfort, they are satisfied, and do not care to submit to a further operation, no matter how trifling, in order to increase their power of vision fractionally—no, not even to enable their surgeons to improve the statistics of their operations. In hospital practice our patients are always eager to get to their homes, as soon as they find themselves allowed to leave their ward for the day-room, and to take a walk in the garden; and it is not easy to submit more than a few of them to secondary capsulotomy.

With regard to the statement, now so often made, that the power of orientation is better in persons operated on by the simple method than where an iridectomy has been done, I would say that this may be the case if wide iridectomies are in the minds of those who make this statement, but my patients labour under no difficulty of orientation once they become accustomed to their spectacles.

In conclusion, I may say a few words with reference to the state of the general health required, in order that the operation for cataract may be successfully performed. In my opinion, there is hardly any chronic disease which contra-indicates this operation, and, as ether or chloroform have become unnecessary, as the operation with cocaine is almost painless, as it entails but a short confinement to bed, and as it is the only means of restoring sight in these cases, it should be done, whenever the state of the eye renders its prospects good, and the patient's life is likely to last for even a few months longer. Nor do very advanced years form a contra-indication. I have frequently operated on persons over 80 years of age, and in them always with a good result. I know no more pitiable sight than that of an aged and infirm person, with bright mental faculties, who is doomed to pass the last years or months of life, deprived of the joy of seeing those who are nearest and dearest; nor is there anything of which I feel more certain than this, that a successful cataract operation gives a renewed lease of life, by reason of the improved psychical condition which follows upon it. I do not wish to convey that the state of the patient's general health is a matter of absolute indifference to me; but, merely, that I do not decline to operate on account of serious organic disease of a chronic nature, which, so far as can be known, may not have a fatal termination for months, or, perhaps, for years to come. I have operated successfully in a case of pronounced Bright's disease, as also in the presence of diabetes. Nor does chronic disease of the heart, liver, or lungs, even when they are all present in the same individual, cause me to decline to operate.

ON FIBROUS STRICTURE OF THE ŒSOPHAGUS.

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[Read in the Section of Surgery, March 28, 1890.]

In 1882 I had the honour of laying before the Surgical Society of Ireland the details of a case of cicatricial stricture of the œsophagus, which I treated by rupture, followed for some time by the passage of bougies. I then entered at some length into the whole subject of fibrous stricture of the œsophagus, its causes, usual situations, symptoms, pathology, and treatment. I do not therefore propose, on the present occasion, to cover the same ground, or to weary you with "reiterated logic," but rather to give the details of some cases which have been, from time to time, under my care, and shortly to discuss the different methods of treatment adopted.

The first case to which I shall refer is the one to which I have already alluded, and to which I should now like to call attention, as I have had during the past year an opportunity of seeing the patient, and of estimating the value of the treatment after the expiry of more than eight years.

Case I.—The patient was a girl, aged twenty, who came under my care, in the Throat Hospital, on the 17th October, 1881, suffering from a stricture in the œsophagus, situated about three-quarters of an inch below the cricoid cartilage. The cause assigned was the impaction of a hard bread crust in the œsophagus four and a half years previously. Dysphagia had come on very gradually, and when I first saw her, even fluids were swallowed with considerable difficulty. She had lost very considerably in weight, and was anæmic and emaciated. The day after her admission I tried to get in a No. 8 catheter (English gauge). Great difficulty was experienced, as the œsophagus was much dilated above the

stricture and I had to probe about against the floor of this dilatation with the olivary point of the catheter before I could find the opening. At last, and after many attempts, I succeeded in worming the catheter through, and I then left it *in situ*. It was retained for an hour, during which period the patient sat on a stool, leaning over a basin, while stringy mucus and saliva in great quantities poured from the mouth. After its withdrawal she experienced great relief, and shortly afterwards was able to swallow some beef-tea with more ease. This gradual process of dilatation was continued daily, and on the fourteenth day I was able to pass a No. 12 catheter through the stricture. Two days later I passed Otis's dilating urethrotome, without the blade, and dilated the stricture to the full size of the open instrument. Immediately after its withdrawal I introduced an ordinary œsophageal bougie (about three-eighths of an inch in diameter). This was retained for several hours. From this date, November 2nd, dilatation was continued by means of Mr. Tufnell's conical rectal bougie, and seven days later, November 9th, I was able to get in the full-sized bougies, the part which was held in the stricture measuring five-sixths of an inch in diameter.

This large bougie was passed daily till the 22nd of December, and retained each time for periods varying from one to four and a half hours. She was then discharged, and went to a situation in the country.

I have frequently heard of her since, but last year I had an opportunity of examining her, as she came up to Dublin, because she thought the stricture was again contracting. I was then very gratified to find that though there was some contraction it was very slight; it was, however, sufficient to make the passage of the full-sized rectal bougie a matter of great difficulty. I then determined to try the effects of electrolysis, and after two or three séances, I found that this electrode, measuring two-thirds of an inch in diameter, passed with the greatest ease. The rectal bougie could also be introduced with ease, so I sent her back to the country.

I think we may look upon this case, though not absolutely, yet to all intents and purposes, cured; as recontraction was so very slight after the lapse of eight years.

Case II.—The second case was that of a lady, aged thirty, the daughter of a doctor in the North of Ireland, whom I first saw on

December 8th, 1883, in consultation with the late Dr. Robert M'Donnell, at whose request I undertook the treatment. The history of her case was as follows:—She stated that in the Spring of 1870, nearly fourteen years previously, she had an attack of acute tonsillitis. The tonsils were cauterised with nitrate of silver, and a gargle was ordered for her, of the strength of which she complained very much. Shortly afterwards she experienced symptoms of œsophageal obstruction, for which her father consulted a doctor in a northern town. Both the patient and her father declared that he passed a bougie very roughly, and hurt her very much, and that she was unable to swallow solid foods for days afterwards. Subsequently to this she experienced recurring attacks of dysphagia, lasting for three or four days, with intervals of three weeks or so, during which time she was nearly quite well. Gradually the intervals became shorter and the periods of dysphagia longer. In 1879, being in Manchester, she consulted a doctor there. He, believing the stricture to be spasmodic, galvanised the œsophagus every second day for weeks. He also tried to pass a bougie, but found great difficulty, and much pain was caused. She could not swallow anything solid for weeks afterwards, even when minced fine. From that time until I saw her with Dr. M'Donnell, no attempt of any kind had been made to remedy her condition, which on December 8th, 1883, was as follows:—For years she had not been able to swallow anything solid. Her diet consisted of beef-tea, thin arrowroot, milk, and sometimes bread soaked in tea. Her breakfast, which consisted of two cups of milk, took her an hour and a half to swallow; luncheon about an hour, and dinner between one and two hours. The food, which was taken in very small quantities at a time, did not regurgitate, but when swallowed took a long time to go down. Eructations occasionally came on, causing great pain referred to the sternum and between the shoulders. She was given a little water to swallow, and a stethoscope (placed at the back, on a level with the third dorsal vertebra and a little to the left) revealed a peculiar scraping sound which could even be heard sometimes at a little distance; but the normal *glou-glou* was quite absent. Dr. M'Donnell and I both made several attempts with variously-sized bougies to pass the stricture, but every attempt failed. We were both quite satisfied that the stricture was organic and cicatricial, and situated in the upper part of the œsophagus, about an inch below the cricoid cartilage.

On the 13th of December she came to my own house, and then,

after many attempts, I at last succeeded in introducing a No. 1 urethral bougie, which was firmly gripped. I left it *in situ* for a few minutes, when an attack of laryngeal spasm came on, which obliged me to withdraw the bougie, and which was quickly relieved by nitrite of amyl. Two days later, No. 1 was again passed and retained ten minutes, no laryngeal spasm resulting. On December 17th, two days later, a fine bulbous-pointed bougie, No. 3, was got in and retained sixteen minutes. On the 19th, the same size was retained seventy-five minutes. The same bougie was passed every second day until the 24th. My note on that day was:—"She does not think she can swallow more quickly, but she can do so with less pain and spasm, and has no choking fits now which she used to have frequently." On that day Mr. J. K. Barton saw her with me. He entirely concurred in the diagnosis, and we then discussed the advisability of nicking the stricture with Maisonneuve's urethrotome, but this was never done, as the gradual dilatation seemed to be progressing satisfactorily. I will not weary you with a daily record of the progress made. I will only call attention to the chief incidents as they occurred in their order, merely saying that by slow degrees the size of the bougies was increased. On the 31st December, 1883, No. 5 was got in. On the 2nd January, 1884, I find this note:—"It is a remarkable thing that when a tight-fitting bougie is first attempted, it always brings on retching, often violent, but a bougie which is only moderately tight, or is loose in the stricture, never does." By the 30th January I had attained to No. 14 urethral bougie, English gauge. On the 13th February I began the use of Tufnell's rectal bougies, and passed No. 3. On the 5th May, 1884, I passed Tufnell's full-size rectal bougie, and continued its use with occasional intermissions until July the 4th, when she went home.

During this period, extending over nearly seven months, the bougies were passed almost without intermission every second day; the urethral bougies were retained for periods varying from ten minutes to three hours at a time, during which time large quantities of stringy mucus poured from the mouth. The rectal bougies, on account of their size, could not be retained so long, but they were kept in for periods varying from twelve to forty-five minutes.

The symptoms, as we might expect, improved, *pari passu*, with the enlargement of the stricture. Thus, on the 13th January, when the stricture accommodated a No. 9 bougie, her husband estimated that an hour and a half had been saved at her meals per day. On

February 25th, she wrote to me to say that her dinner had consisted of roast duck, cut fine, and mashed potatoes; this was the first solid food she had eaten for years. When she left Dublin on July 4th, she could eat whatever she liked. She has visited me frequently since, and about twice a year I pass a bougie for her, but there is apparently no recontraction of the stricture; this is probably due to the fact that she is herself alive to the danger of recontraction, and accordingly comes at intervals to get the bougie passed.

Case III.—The next case is that of a lady, aged forty-three, who consulted me for the first time on the 18th of June, 1886, for difficulty in swallowing; and it is especially interesting, as it was the first case of œsophageal stricture in which I tried electrolytic treatment. She had suffered from gradually-increasing dysphagia for fifteen years, for which she could assign no cause. When I saw her she was extremely emaciated, markedly exsanguine, and of highly nervous temperament. This, I presume, was the reason that her dysphagia had always been looked upon as spasmodic, and had been treated invariably by tonics. She could swallow fluids with comparative ease, but any attempt at solids brought on attacks of choking, which generally lasted an hour. Bread she found the easiest solid to manage, and meat was always the worst. The first day I saw her, I tried to pass an œsophageal bougie but failed, and then I tried a No. 12 urethral bougie, but this was also unsuccessful, as I could not get it past a smooth, tough obstruction which was localised to a spot one inch below the level of the cricoid cartilage. The result of my efforts was severe pain in the back of the shoulders and in the chest. She was, however, quite well until the next morning, when a small piece of bread brought on an attack of choking, which was followed by violent spasms and retching, which lasted the whole day. I made no further attempt for four days, and then, on the 22nd June, I succeeded in getting an olivary-pointed gum-elastic bougie (No. 10) through the stricture. I then found that the stricture was apparently due to a ring of smooth, tough, fibrous tissue. The next day the same bougie was passed and retained ten minutes. On the following day, June 24th, I passed an electrode down to the stricture, and connecting it with the negative pole of the battery, passed a weak current, 9 Leclanché's cells for ten minutes, after which a No. 16 electrode passed easily. The electrolysis caused very little distress. On

the 25th the same electrode was used for about ten minutes, and again for eight minutes on the 26th. On the 28th I passed No. 20 electrode down to the stricture, but could not get it through. I then turned on the current, four times of two minutes each. At the third the electrode passed through the stricture with ease. On the 29th and 30th the same electrode was used. The treatment was now suspended until the 5th July on account of the supervention of the catamenia, which were always abundant and necessitated her keeping to her bed. On that day I found the No. 20 could pass easily, merely hitching at the stricture; but the current was not used. At this time she could swallow, she said, without difficulty, her throat felt so large. She was obliged to return home to the West of Ireland for family reasons, so that I did not again see her until the 25th of the following October. I then found that there was some recontraction. I could just get an electrode three-eighths of an inch in diameter through the stricture. The following day I electrolysed the stricture for ten minutes, and succeeded in passing a larger sized electrode, five-elevenths of an inch in diameter. On the 30th October, after electrolysing the stricture with 12 cells for a few minutes, I passed the largest electrode I possessed, measuring two-thirds of an inch in diameter. On Nov. 1st and 2nd electrolysis was employed for some minutes each day, the largest electrode being used, and on November 3rd, 1886, she returned home.

I have seen her at long intervals since, but I have been unable to detect any sign of recontraction. I saw her last year and was surprised to see how fat she had grown, and how improved she was in her general health. She never experiences now the slightest difficulty in swallowing her food. It is worth while observing that from the day I first saw her, on June 18th, until she returned home on November 3rd, she visited me sixteen times, and electrolysis was employed on eleven occasions. This forms a striking contrast with the time required for gradual dilatation by bougies alone.

Case IV.—The fourth and last case I have to record is that of a man, aged sixty-eight, by occupation a railway guard, who has recently been under my care. Gradually increasing dysphagia had been coming on for seven or eight years. At the beginning there were intervals during which he could swallow quite well. He

could assign no cause for the disease, and never had had syphilis, or any previous disease or injury to the gullet. Until three years ago he was able to eat his ordinary food, but then he had to begin to mince his food. In May or June last he had to give up eating meat or any solid food. His food lately has consisted of the soft part of bread broken into cocoa, soup, or chicken broth with bread soaked in it, and for supper Benger's food. This he could swallow well, but nevertheless he had been emaciating rapidly. When he came to me on the 17th of December last, I found I could pass a fairly large-sized olivary pointed bougie (somewhat less than half an inch in diameter) through an apparently fibrous stricture, situated, as nearly as I could measure it, about four centimetres from the orifice of the œsophagus. On passing a bougie downwards, a second stricture was discovered close to the cardiac orifice of the stomach. A series of measurements were taken, which showed that there was a distance of 15½ centimetres between the two strictures; and therefore the lower stricture was situated about four or five centimetres above the cardiac orifice. At this time I could not estimate its size, as I could not succeed in passing any bougie through it. I therefore directed my attention during the next few days to dilating the upper stricture. On the 21st December his weight in his clothes was 10 stone 5½ lbs. On December 24th I passed this œsophageal dilator, made by Matthieu, through the upper stricture, and having dilated it to its full size (about one inch in its broadest diameter) I withdrew it slowly. The subsequent pain was very slight, and he told me he thought he could get down his food more easily.

Up to the 30th December I had not been able to pass even a small bougie through the cardiac stricture, and I discussed both with the patient and with Dr. Ford, of Waterford, who sent up the patient to me, the propriety of dilating this lower stricture with the finger, through an opening made in the stomach, as recommended, and successfully performed, by Loreta, of Bologna; but on this day I succeeded in passing this œsophageal bougie. I then determined, if possible, to forcibly dilate the stricture, and accordingly, having succeeded in passing the same œsophageal dilator as before, I opened it until it measured about three-fourths of an inch in diameter, and withdrew it. The stricture was found to be extremely firm and resisting, and the dilating process caused some pain, which continued during the night, and was referred to a spot between the shoulders. The following day deglutition seemed to

him to be easier, and I then succeeded in passing a No. 20, and on January 1st a No. 28 into the stomach. This No. 28 measured one and a half inches in circumference. His weight was again taken, when it was found that he had gained 1 lb. 3 oz. since December 21st. The bougie was kept in for fifteen minutes.

On January 3rd I began electrolysis, and repeated it every day until the 9th, and again on the 13th and 16th—that is, in all, nine times. Both strictures were electrolysed for about fifteen minutes each time. On the 10th of January I was able for the first time to pass this full-sized œsophageal bougie through both strictures into the stomach. It was passed daily till the 17th, when I saw him for the last time; on each occasion it was kept in from fifteen minutes to half an hour. On January 4th he was able to take meat for the first time, and continued it daily afterwards. His weight on the 17th January was 10 stone 11 lbs.—that is, he had gained since the 21st December 5½ lbs. in weight. He returned home on the 18th January. He had gained also considerably in strength, and the day before he left me he told me with pride that he had walked the whole way from Phibsboro'-road to my house.

Dr. Ford, of Waterford, who has been attending him since his return home, and who has passed the bougie successfully every second day, wrote to me on the 13th of February saying, "Ned could not be better. He saw you on December 17th, and since then he has gained 18 lbs. in weight. His appetite, whetted by two or three trips to Tramore every day, is excellent, and he has no difficulty in deglutition. . . . This morning I noticed his uniform coat fitting him as it should. There was a time when it hung loosely on him."

I heard from the patient himself on the 13th of March. He tells me there is no difficulty in passing the bougie down into the stomach. This is done every second day, and left in for fifteen minutes each time. He says he gained 28 lbs. between the 21st of January and the 13th March.

These four cases of fibrous stricture of the œsophagus are instructive in many ways. The last one, of course, is open to the objection that the treatment has been too recent to speak with any confidence as to the ultimate result, and I also confess that, for a time, I was impressed by the doubt as to whether some malignant disease were not at the root of it, having regard to the age of the

patient—viz., sixty-eight. But, on the other hand, a history of gradually increasing dysphagia extending over a period of seven or eight years, and the improvement which followed the process of dilatation, are in favour of the strictures being of a cicatricial nature. Added to these the sense of touch, which gave the impression of a dense band or thickening instead of the nodulated tumefaction of a malignant growth, I think I am justified in looking upon the case as one of a benign character, although the possibility of a malignant neoplastic growth cannot be entirely dismissed from view.

In the first place these cases show that forcible dilatation or rupture, when followed by systematic treatment, may be useful in some of these cases. Its advantages are that it hastens the process of dilatation, especially in cases where it is desirable to open up the passage with as little delay as possible; and secondly, it is a much safer proceeding than dividing the stricture with a sharp instrument. There can be little doubt that division of a stricture, where we can regulate the amount, position, and direction of the incision, is ideally the best, but unfortunately in the œsophagus this is not exempt from danger. In Mackenzie's book on the throat and nose he gives statistics of eleven cases of cicatricial stricture in which internal œsophagotomy was performed, with three deaths, all due to the operation. This gives a percentage of 27·28 per cent. Rapid dilatation, on the other hand, is comparatively a safe procedure, and has proved beneficial in the limited experience I have had of it. Electrolysis, I believe, will prove of great service in these cases which are acknowledged to be difficult and often intractable to treat. Our experience of its use in œsophageal strictures is, of course, very scanty, but in similar affections of the urethra it has found many warm supporters. Many will remember the interesting paper on this subject read before this Academy by Mr. Patrick Hayes. It was mainly owing to that paper that the idea occurred to me to try it in œsophageal strictures of a cicatricial nature. Case III. seems to show that by electrolysis we can obtain a more rapid dilatation than by the simple passage of bougies, and in her case certainly the improvement appears to be permanent, as there was

no recontraction three years afterwards. In the last case I also think it was of use, and I think the enlargement of the cardiac stricture especially was hastened by the use of the electric current.

There are other points of interest which time will not allow me to refer to now—but I trust that other surgeons will be induced to try electrolytic treatment of these strictures, and to publish the results, so that by accumulated knowledge we may be able to arrive at a just estimate regarding its utility.

"Organic stricture, of whatever kind, is incurable. *Dilatation*, by means of bougies increasing in size, may afford partial and temporary relief. . . . The principal use of a bougie is to determine the existence of a stricture, its situation and nature, and thus to complete the diagnosis of this affection—due caution being observed in passing the instrument."[a]

[a] Gant's Student Surgery, 1890. P. 536.

A RARE CASE OF THE CONGENITAL FORM OF RANULA.

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[Read in the Section of Surgery, March 28, 1890.]

In a paper read before the Moscow Medical Society by Dr. N. Muller, on "Ranula in Newborn Children," he states that in the Foundling Hospital at Moscow four or five cases of congenital ranula had been observed during a period of seven years in about 80,000 children; and the *London Medical Record* of December, 1877, makes mention of the fact that, up to that period, there were only two known instances of this affection on record—one published by Dubois in 1833, and a second, of more recent date, by M. Lombard. Mr. Bryant records two cases; both were, probably, he states, congenital, and came under his notice when the patients were about twenty years old. Sir W. Ferguson records one case. Ranula was formerly thought to be due to an obstruction of the salivary ducts; but recent authorities seem to favour the theory that it is caused by obstruction of one of the mucous glands that are situated beneath the tongue. Fairlie Clarke, in his work on "Diseases of the Tongue," speaking of the larger tumours that are occasionally, but rarely, found lying between the tongue and the lower jaw, says that, "Although the term ranula is applied to them, they are analogous to the sebaceous tumours which are so frequently met with on the skin, and, like them, contain a thick gritty, putty-like substance, somewhat offensive in odour. The mucous membrane beneath the tongue is, furthermore, often the seat of cystic formations, which take their origin sometimes in the ducts of the sublingual and submaxillary glands, sometimes in the areolar spaces, and, possibly, also sometimes in the bursa, between

the genio-hyo-glossi muscles. To all these cystic growths, whatever may be their exact pathology, the term ranula is applied."

The case of ranula that I desire briefly to bring under the notice of this Section of the Academy of Medicine resembles that class of tumour noticed by Fairlie Clarke as analogous to the sebaceous tumours found on other portions of the body. It presented some peculiar and interesting features which I deemed worthy of recording, more especially as the growth was noticed *on the second day after birth, and the subject is now very nearly twenty-nine years old.*

CASE.—This patient was admitted into St. Michael's Hospital, Kingstown, on March 19th, 1889, as a very urgent case, requiring immediate relief as he had extreme difficulty in breathing, and had difficulty in swallowing even liquids for forty-eight hours previously. Certainly, the appearance of the patient was alarming. He showed great prostration; his appearance was very anxious; his countenance bore a dusky hue, and he had extreme dyspnœa; his mouth was wide open, his pulse was weak, and he was bathed in a profuse perspiration. On examination, at first sight, he gave one the impression that he was suffering from acute glossitis. It was with great difficulty he could articulate, and it was impossible to understand what he said. The tumour very nearly filled the entire cavity of the mouth, and the tongue was pushed upwards and backwards, and could hardly be felt with the tip of the finger. The tumour also projected beneath the jaw into the mylo-hyoid space, and assumed an elongated shape in this situation, being about five to six inches in length; it was hidden from view by the patient's beard, and was as large as a good sized orange. The projection into the cavity of the mouth commenced to cause inconvenience about some eight months prior to the date of his admission to hospital, and for over a month he had experienced difficulty in swallowing, and subsisted principally on liquid nourishment. He daily essayed to get some particles of solid food down, but it was quite an ordeal to do so, as it required a good deal of manipulation to get the food to the back of the mouth. There was a continual dribbling of saliva, and for a week previous to my seeing him he was unable to sleep or lie down in a recumbent position for fear of suffocation. On examining the tumour, fluctuation was quite evident in the mass in the mouth, but in the neck it partook more of a solid nature.

The treatment that suggested itself was to aspirate that portion of the mass within the mouth, which was done soon after his admission. Nearly fifteen ounces of fluid of a creamy nature was drawn off, and gave him at once great relief. He could now speak more distinctly, but found it difficult to move his tongue forward. The tumour rapidly filled again in the mouth, and was aspirated a second and a third time, large quantities of a similar fluid as before being drawn off at each aspiration. The mass now in the neck became softer, and deep-seated fluctuation could be detected. It was then decided to lay open the tumour from the neck, and I made a deep incision into it, which gave vent to some four ounces and a half of thick brown pultaceous matter, rather offensive in odour. I then scooped out the remainder of the cavity, and took away a good deal of cheesy and grit-like substance. After this the cavity suppurated, and there was a free discharge of pus for a week or so. A short time subsequently the patient left the hospital freed from the unsightly mass that had disfigured him for so many years.

In the case under notice, the patient states that so long as he can remember he had a swelling about the size of a small Spanish nut lying beneath the tongue, which caused him little or no inconvenience, excepting that, at some times more than others, he had a slight difficulty in articulating plainly. The parents both assure me that they noticed this swelling when their son was but a day or two old. It gave them very little concern, as it appeared so small, and did not seem to interfere with the movements of the tongue, the child being able to suck freely, and protrude the tongue. At this time it was about the size of a small pea, and remained so for some years, then got a little larger, increasing to the size of a small nut. It then remained stationary, until some fifteen months ago, when it commenced to increase rapidly in size, and interfered with the movements of the tongue. No assistance or treatment was ever sought for, as the sufferer had a dread of surgical interference; and if it were not for the fact that he went within measurable distance of both starvation and suffocation he would, probably, not have given his consent to any ameliorative measures being adopted.

I may mention there was a free connection between the mass in the mouth and that in the neck below the jaw.

THE MASTOID REGION OF THE SKULL, WITH SPECIAL REFERENCE TO OPERATIONS ON THE PART.

By AMBROSE BIRMINGHAM, M.B.;
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[Read in the Section of Surgery, May 9, 1890.]

Some two years ago an important discussion took place in the Royal Academy of Medicine in Ireland upon the operation of trephining in mastoid and tympanic disease. There was chiefly under consideration an operation proposed and practised by Mr. Wheeler, of Dublin, in which the skull was trephined above and behind the ear, the trephine being placed in front of a vertical line dividing the mastoid process, and having its lower margin on the level of the external auditory meatus. By this means, Mr. Wheeler pointed out, the mastoid cells and tympanum might be drained, and the cranial cavity at the same time opened to give exit, if it were found necessary, to intracranial collections of pus. There were many at the meeting who warmly advocated Mr. Wheeler's operation, and others with whom it found no favour. One of the chief objections urged by the latter against it was, that if his directions were followed the lateral sinus would be opened into. At the time I was unable to come to a satisfactory conclusion on this point, for I knew that the lateral sinus varied to a marked degree in different skulls, both in size and position; and I felt that it would be useless to draw conclusions from the examination of a few specimens, consequently I began to collect material for an investigation into the matter. Owing to the fact that this part of the skull is generally sawn up during dissection I found it difficult to obtain a satisfactory number of specimens, but at length I have

collected one hundred, and I feel that from an examination of this number some useful conclusions may be arrived at.

Although I started solely with the intention of ascertaining whether the anatomy of Mr. Wheeler's operation was sound or otherwise, as I went along I found that several side issues sprang up, and some of these appeared so important that they were followed with more attention and interest than the original point of investigation. It will be better to consider first these side issues, for they will be found to aid markedly in the solution of the original question.

As a preliminary it was necessary to select some fixed landmarks upon the skull from which to make my measurements. The first point selected was the centre of the bony external auditory meatus. Occasionally it is a little difficult to decide exactly what the centre is, owing to the ill-defined nature of the upper and posterior borders of the meatus; still, a small amount of care will always lead to a correct solution of the question in these cases.

The walls of the meatus also vary to a considerable degree, both in surface and direction. In the majority of cases the upper wall is nearly horizontal, with a slight slope downwards at its inner part; occasionally this downward slope is well marked. The posterior wall is inclined strongly forwards and inwards, with few exceptions. The anterior runs in with a tendency forward at its deeper part. The floor is always convex, dipping externally and internally. The general direction of the bony meatus seems to be in the majority of cases nearly horizontally inwards and forwards.

Secondly, an easy guide to the horizontal plane was also required. This was found in the line of the upper border of the zygoma, which runs practically horizontally forwards in the majority of cases, with just a slight tendency upwards at its anterior part in some specimens. It will generally be found a useful guide.

A third guide selected was Mr. R. W. Reid's "base line," running from the lower margin of the orbit backwards through the centre of the bony external auditory meatus. I wished to find where this line would terminate posteriorly, particularly with regard

to the external occipital protuberance. With this object I examined twenty skulls. The following was the result:—

Base line from ¼ to ½ inch below protuberance	-	10
„ from ¼ to ½ inch above „	-	4
„ corresponding to protuberance	-	6

I also examined the relations of the base line to the superior curved line. Result:—

Base line below superior curved line	- - -	12
„ above „ „	- - -	2
Superior curved line, corresponding pretty closely to base line	- - - - - - - -	6

From the foregoing it will be seen that, as a rule, both the external occipital protuberance and the superior curved line lie above the level of the base line to a slight extent. Half an inch measured here on the surface of the skull means a very small difference of *level*, owing to the almost horizontal position of this part of the cranium.

It now appeared desirable, and even necessary to map out on the surface of each preparation two important points—namely, the position of the lateral sinus, and the level of the roof of the tympanum and mastoid antrum (that is, of the upper surface of the petrous portion of the temporal bone at its outer part), in such a way that their relations to the mastoid region might be seen at a glance. The following method was adopted:—A fine drill was driven from within through the bone, at short intervals, along the course of the lateral sinus from its beginning to the point at which it bends inwards towards the jugular foramen, care being taken to hold the drill at right angles to the sagittal plane of the specimens, and that the holes were made regularly, a fixed distance from the margins of the sinus. When the exterior of the skull was bored the holes were quite distinct, and by their aid an exact representation of the lateral sinus was drawn clearly on the exterior of every specimen, so that a glance showed its relation to the meatus, &c. By the same method I found the level of the upper surface of the petrous portion of the temporal, not where it actually abuts against the vertical plate of the squamous, but some distance from

this plate, where it forms the roof of the antrum mastoideum and inner part of the meatus (from which it is separated by cells). This I also marked distinctly on the exterior of the bone, and in this way the limits of the space in which the antrum might be sought without injury to the sinus or the brain were clearly shown.

With regard to the level of the roof of the antrum, formed by the upper plate of the petrous, I found that this varied chiefly in two ways—first, in its height above the upper margin of the meatus; and, secondly, in the direction of the plate of bone itself. As a rule, this plate is approximately horizontal (57 per cent.) It frequently is raised posteriorly, sloping backwards and upwards (32 per cent.), or it may be arched or irregular (11 per cent.).

In height, measured vertically over the middle of the meatus, the distance varied considerably. From the meatus, just internal to its upper border, to the line of the roof of the antrum marked on the exterior of the skull, it was

$\frac{1}{16}$ of an inch in		2	per cent.
$\frac{2}{16}$	"	25	"
$\frac{3}{16}$	"	27	"
$\frac{4}{16}$	"	27	"
$\frac{5}{16}$	"	3	"
$\frac{6}{16}$	"	16	"

This shows a variation from $\frac{1}{16}$ of an inch in 2 per cent. to $\frac{3}{8}$ in 16 per cent., with an average slightly under $\frac{1}{4}$ of an inch.

It is important to bear in mind the variation which is here shown in the level of the roof of the antrum, in all operations which seek to open the antrum without entering the cranial cavity, and, on the other hand, in operations intended to open both cranial cavity and mastoid cells at the same time, as in that of Mr. Wheeler. In these cases it will be wise to remember that the petrous may extend to a height of $\frac{3}{8}$ of an inch above the meatus in a considerable proportion of skulls, but that it may in others fall barely above the level of its roof. The height is usually about $\frac{1}{4}$ of an inch, and as the plate of bone generally lies nearly horizontally, the level is almost the same immediately behind the meatus

as directly above it. As already explained, the height here given is that of the upper surface of the petrous some short distance from its junction with the squamous. At the junction of the two the petrous is often prolonged upwards in a sloping fashion for some distance, but this must be disregarded when considering the lines to be adopted as guides in perforating to a depth such as that at which the antrum lies.

From the foregoing it will be seen that if, in *every* possible case, we wish to avoid the cranial cavity when drilling into the antrum, the instrument should not be placed above the level of the extreme upper margin of the meatus, otherwise the cranial cavity will be occasionally opened.

By the method previously described I examined the position and relations of the lateral sinus mapped on the exterior of the skull in a hundred specimens with, I think, pretty definite results. It will be best to quote the accounts given of the surface marking for this sinus in some of our best known text-books, and then point out in what my results agree with or differ from these.

Few text-books give a careful account of the direction and topography of the sinus. In Quain, volume I., page 668, we find: "From the latter point (*i.e.*, external occipital protuberance) the lateral sinus runs horizontally outwards and forwards to a spot about an inch behind the external auditory meatus, where it turns downward towards the mastoid process." Treves and Owen give a similar description. Professor Windle, in his work on "Surface Anatomy," says that a line from the external occipital protuberance to the point of the posterior inferior angle of the parietal bone—the asterion—will mark the position of a part of the lateral sinus. "At the asterion," he says, "it turns down and runs behind the posterior border of the mastoid process to the base of the skull."

These are the best descriptions of the topography of the sinus which I have met with. Before criticising them I wish to point out that the relations of the lateral sinus to the surface are extremely inconstant, so much so that it is impossible to lay down any method of arriving at the position of the sinus accurately in its whole extent. These irregularities are due, to a certain extent,

to varieties in the curves of the sinus, but to a great extent also to irregularities in the bones. Regarding the description in Quain quoted above, it is correct to say that the sinus, as a rule, corresponds posteriorly to the level of the occipital protuberance. I have found it lie at this level in 50 per cent. of the cases examined, below it—but never more than ½ an inch—in 33 per cent., and above it in the remaining 17 per cent. Beyond this the description in Quain, adopted by Treves and Owen, differs completely from what I have found. I would say the sinus never runs *horizontally* forwards and outwards from the occipital protuberance. On the contrary, it forms a distinct and well-marked arch, with a height varying up to an inch. The anterior extremity of this arch corresponds to the point where the sinus turns in to the base of the skull—a point about ¼ or ⅙ of an inch below the level of the floor of the meatus and a variable distance behind it; the posterior end is at a point somewhere about the occipital protuberance. Usually the posterior end of the arch is on a higher level than the anterior by about half an inch. From the occipital protuberance—which, it will be remembered, in most cases lies above the base line—the sinus arches gradually upwards, sometimes more quickly, sometimes more gently, until it reaches its greatest height at a point found, on an average, 1½ inches behind and about ⅝ of an inch,[a] or a little more, above the centre of the external auditory meatus. Here it turns down gradually or abruptly, and lies just in front of the posterior margin of the mastoid process and a line prolonging that margin upwards and backwards; finally it turns in towards the base of the skull below the level of the meatus.

As regards the bend of the sinus where it turns down at the asterion, it may vary within wide limits. It may be merely a gentle curve, so gentle that it is hard to say where the dip down really commences; on the other hand, it may be very sharp, presenting a distinct apex or knuckle; or it may be anything between these two conditions. Further, the height of this bend above the base line is very variable; in some cases it is only $\frac{3}{16}$ of an inch, in others it reaches ⅞ of an inch, measuring to the middle line of

[a] Measuring to the middle line of the sinus.

the sinus—which means that the upper margin of the sinus is sometimes found more than an inch above the posterior part of the base line, although this part of the base line is considered to correspond to the transverse fissure of the brain, that is, to the lateral sinus. As a rule, the sharper the bend of the sinus the greater is its height. The average height above the base line is over $\frac{5}{8}$ of an inch, measuring to the upper margin of the sinus at the bend. These measurements are of importance in trephining for temporo-sphenoidal abscess according to the lines laid down by Mr. Barker.[a]

From the bend the sinus runs downwards and forwards on the inner surface of the mastoid at an angle of about 60°; and in the opposite direction it runs downwards and backwards on the occipital in a gentle slope. The curve, taken altogether between the two extremities, resembles that of an Italic *f* placed horizontally ⌒ with the anterior curve well marked and concave inferiorly.

Now to localise these parts of the sinus on the surface. The lower and anterior end—the point where the sinus turns in to the base of the skull—will be found from $\frac{1}{4}$ to $\frac{1}{6}$ of an inch below the level of the lower margin of the meatus and somewhere in front of the posterior margin of the mastoid. The apex of the curve, as just pointed out, usually lies $1\frac{1}{2}$ inches behind and $\frac{5}{8}$ inch above the level of the centre of the meatus.[b] The posterior end lies at or immediately below the level of the external occipital protuberance, but it would be unwise to consider that it will be invariably found at this level. It should to be given a full $\frac{1}{2}$ inch below and as much above the point of the protuberance as its field of wandering. It ought to be also pointed out that very frequently the sinus turns up into the superior longitudinal or straight sinus before it reaches the protuberance.

To localise the portion of the sinus between the posterior end and the apex of the curve, probably the simplest line is that given by Professor Windle; a line from the protuberance to the asterion

[a] Manual of Operative Surgery; and elsewhere.

[b] Although this is the position of the "apex" when marked, in those cases which have only a gentle curve the sinus may reach to about the same height for an inch further back.

usually corresponds fairly well to this part of the sinus. If the asterion cannot be made out in the living subject, then we must fall back on the relation of the sinus to the curved lines of the occipital and the base line of Reid.

The superior curved line is a fair guide to the sinus *at its posterior part.* I have seen the sinus ½ an inch above the curved line, and, on the other side, ½ an inch below. Nevertheless, in the majority of cases, the sinus may with safety be sought for at or immediately below the superior curved line, within an inch of the occipital protuberance. (At the curved line, 65 per cent.; below, 27 per cent.; above, 8 per cent.)

The relation of this part of the sinus to the base line is most variable. In 57 per cent. it lies entirely above the base line, in 29 per cent. at the base line posteriorly and then arches upwards and forwards to the apex, and in 14 per cent. it lies below the line posteriorly—that is, in 57 per cent. of skulls the sinus, from its posterior end to the part lying behind the meatus, is placed bodily above the base line; in another 29 per cent. it is at the same level behind, but above it in the rest of its course to the meatus. Treves gives the following direction for mapping out the sinus, in his "Surgical Anatomy"—a book of which I cannot speak too highly:—"The lateral sinus, whose course is represented by a line drawn horizontally from the occipital protuberance to a point about 1 inch behind the external meatus of the ear, where it turns downwards, to groove the mastoid process." Owen, in his late manual, gives similar directions. A glance at my diagram will show at once that such a line, in the majority of cases, gives no idea of the true position of the sinus. I have pointed out above that the upper margin of the sinus will in some cases be found over an inch, and, in the average condition, ⅝ of an inch from this line at a point where it is of the utmost importance that it should be properly localised, namely, near the seat of trephining for temporo-sphenoidal abscess.

Though I was happily able to agree with Professor Windle in his method of localising the portion of the sinus just treated of, I fear I must differ from his description of the part of it in front of the

bend. Of this part he says—"At the asterion it turns down and runs behind the posterior border of the mastoid process to the base of the skull"—and his is the only attempt in any of the books mentioned to locate this part of the sinus. The examination of my specimens leads me to a different conclusion. I have never seen the sinus run down behind the posterior margin of the mastoid process; in the average condition it runs just in front of this margin, and a line running upwards and backwards from it, but as a rule they are not parallel, for the sinus is somewhat nearer to the vertical than the border. This is the average condition, but I have seen the *posterior margin* of the sinus as much as $\frac{1}{4}$ of an inch in front of the border, and, on the other hand, I have seen occasionally the posterior margin of the sinus lie very slightly behind the border of the process, but, even in these cases, the sinus as a whole lay decidedly in front of the border of the mastoid. With a broad mastoid process the sinus will be more likely to lie some distance in front of the border than with a narrow one, still, even with a very wide process, the sinus may extend quite back to its posterior edge. In connection with this, it may be remarked that the sinus, or more correctly the groove for the sinus, occasionally becomes much broader here where it lies on the mastoid than it is in the rest of its extent.

From the foregoing it is seen that the relation of the sinus to the posterior margin of the mastoid is variable; it is even more variable in its relation to the anterior border of the process, or, more practically speaking, in its relation to the posterior wall of the external auditory meatus, and this relationship is a matter of vital interest in opening the mastoid cells. I have found the anterior margin of the sinus reach to within $\frac{3}{16}$, that is very little over $\frac{1}{8}$th, of an inch of the posterior wall of the meatus; on the other hand, in 3 cases, the distance was $\frac{12}{16}$, or $\frac{3}{4}$ of an inch; the average distance being a $\frac{1}{2}$ inch. This matter and the distance of the sinus from the surface will be more fully treated of further on.

Summarising these facts relating to the position of the lateral sinus, it may be described as beginning at or near the occipital protuberance; from this it runs for a distance along the superior

curved line, gradually ascending from the base line until its upper margin reaches a height of nearly $\frac{3}{4}$ of an inch from that—the base—line at a point an inch and a half behind the centre of the meatus; here it turns down and forwards running on the mastoid just in front of its posterior border, and about half an inch behind the posterior wall of the meatus; $\frac{1}{4}$ inch below the meatus it turns in to the base of the skull.

Having thus mapped out the position of the lateral sinus it would be well to see what relation the seats recommended for trephining in temporo-sphenoidal and cerebellar abscess bear to it. I shall consider the operations proposed by Mr. Barker. He, in trephining for temporo-sphenoidal abscess, directs the pin of the trephine to be placed $1\frac{1}{4}$ inches above and $1\frac{1}{4}$ inches behind the centre of the meatus.[a] A short time ago Mr. Barker's directions were followed in an operation by a surgeon of my acquaintance, and the lateral sinus was exposed in the trephine opening; as well as I remember the trephine was a $\frac{3}{4}$-inch instrument and the sinus occupied nearly the lower third of the opening.

An examination of my specimens shows that if the pin of a $\frac{3}{4}$-inch trephine (outside measurement) be placed at the point indicated by Mr. Barker, the lateral sinus will be exposed at the edge of the opening in about 15 per cent. of cases. I have measured the distance from the base line to the upper border of the sinus, *at a point* $1\frac{1}{4}$ *inches behind the centre of the meatus*; the maximum was $\frac{15}{16}$ inch, the minimum $\frac{5}{16}$, the average $\frac{5}{8}$ inch.

A large posterior branch of the middle meningeal artery is almost always exposed in a hole made, according to Mr. Barker's rules, with a $\frac{3}{4}$-inch trephine, but it will be exposed probably just as often by an aperture $\frac{1}{2}$ an inch higher up.

Further, according to the diagram in his book, Mr. Barker's operation exposes the middle temporo-sphenoidal convolution; this, I fear will not be the case in the majority of heads; the inferior temporo-sphenoidal corresponds, I would say, more closely to the hole.

With the object of avoiding the lateral sinus in all cases, I would

[a] Manual of Surgical Operations, p. 402.

suggest, in trephining for temporo-sphenoidal abscess, that the centre of the trephine should be placed ½ an inch above the point suggested by Mr. Barker—that is, the pin of the trephine should be 1¼ inches behind and 1¾ or even 2 inches above the centre of the external auditory meatus.

For opening cerebellar abscess, Mr. Barker directs the trephine to be placed 1½ inches behind the centre of the meatus and 1 inch below the base line. Such a site is, I believe, perfectly safe so far as the lateral sinus is concerned; but, as mapped out on the dry skull, in some heads it falls very near the mastoid process, and would, undoubtedly, occasionally engage the trunk of the occipital artery as it lies in the groove of the temporal; besides, the bone is here a considerable distance from the surface. On the other hand Mr. Barker states that, at this point, the most accessible part of the abscess, if present, will be reached. As to this I can give no opinion, but, if shifting the position of the trephine half an inch backwards would entail no inconvenience from the surgical point of view, apparently from the anatomical view a more suitable site would be two inches behind the middle of the meatus and an inch below the base line; here the lateral sinus is out of danger, the bone is not far from the surface, and the occipital artery may be avoided.

Of course I make these suggestions solely from anatomical considerations, and with all due respect for the views of such a distinguished surgeon as Mr. Barker, who has done so much for the surgery of the brain.

The surface marking for the transverse fissure separating the cerebrum from the cerebellum, will, of course, be the line of the lateral sinus as far as the asterion, and, as shown above, this agrees in no way with the part of Reid's base line behind the meatus, which is usually given in text books of anatomy and surgery as the marking for the sinus and the fissure.

Since it has become a well-recognised operation to trephine the lateral sinus for septic thrombosis, I have sought for some point at which the anterior part of the sinus might be reached with certainty. This I have found difficult to achieve. If the asterion be adopted,

a trephine here will always reach the sinus, but then the asterion is not easily made out in the living subject, it is too far away from the ear, the seat of the primary disease, and, besides, it is a variable point sometimes at the base line, sometimes as much as $\frac{5}{8}$ of an inch above it (its distance behind the middle of meatus varies from about $1\frac{1}{2}$ to 2 inches). In its varying positions it seems to drag the lateral sinus constantly with it.

The best site I can suggest is at a point $1\frac{1}{8}$ inches behind the centre of the meatus, and on the level of a line drawn horizontally backwards through the extreme upper margin of the bony external auditory meatus; this will be about $\frac{1}{4}$ of an inch above the base line. In every case a hole made with the pin of a $\frac{3}{4}$-inch trephine at this point will expose the lateral sinus. In some few cases the sinus will not be fully shown by this opening, but in the large majority it will be thoroughly exposed. A little difficulty in such an operation may arise from the fact that the trephine hole will overlie the upper and posterior border of the petrous portion of the temporal, which is here prolonged backwards as a projecting ridge. In removing the crown of bone this will offer some opposition.

From what I have seen of the variations of the sinus, and from what I have tried to set forth above regarding it, my advice to those who wish to trephine the skull in this region and to avoid all risk of wounding the sinus would be—if you are to trephine above the sinus, draw a line from a point $\frac{1}{2}$ an inch above the external occipital protuberance to a point $1\frac{1}{2}$ inches behind the centre of the meatus and 1 inch or $1\frac{1}{4}$ inches above it. You may with safety bore the skull above this. If, on the other hand, you wish to trephine below the sinus draw a line from a point $\frac{1}{2}$ an inch below the occipital protuberance to the level of the base line $1\frac{1}{2}$ inches behind the meatus. You may bore below this without endangering the sinus; these lines mark out the extremes between which it varies.

I next turned my attention to the localisation of the mastoid antrum and to some of its relations. All my observations up to the present have been made on adult skulls, consequently I have no remarks to make upon the development of the region and the

changes associated with age. For information on these points I would refer to Dr. Symington's masterly work on the anatomy of the child, and elsewhere. Regarding the anatomy of the parts in the interior of the bones I have little to add to the descriptions found in works on the ear, such as Dr. Buck's. From these we learn that behind and external to the tympanum lies the common sinus of the mastoid cells or antrum mastoideum, communicating with the cavity of the tympanum by a triangular aperture, with sides of about $\frac{1}{8}$ of an inch; that this antrum is usually large enough to contain a small pea, that it is always present, even in the infant, and that into it open the mastoid pneumatic cells, which are found everywhere on its walls except on the roof. These pneumatic cells, Buck tells us, extend, when fully developed, up to within $\frac{1}{2}$ an inch of the temporo-parietal suture. Anteriorly they extend forwards over the auditory meatus; but posteriorly they end abruptly in the neighbourhood of the temporo-occipital suture, extending in some very rare cases even past the suture into the occipital bone. Buck also says, "The lateral sinus occupies a groove on the bone in the immediate vicinity of the posterior limits of the mastoid cells;" and he points out the short distance—3 to 6 mm.—from the sinus to the posterior wall of the antrum.

Symington and Buck quote the result of an examination of 250 adult temporals made by Zuckerkandl, which shows that in 20 per cent. there were no air cells save the antrum, the bones being entirely solid or diploic; in 36·8 per cent. the cells of the mastoid were entirely pneumatic; and in 43·2 per cent. they were partly pneumatic, partly diploic.

In connection with these generally adopted views I would remark that I have found the opening between antrum and tympanum to agree pretty closely with the description given above. Regarding the constancy of the antrum, I have found it completely absent in one case—a solid mastoid; in another solid mastoid there was a fair-sized antrum normally connected, and behind it, but separated from it completely by a plate of bone, another similar but isolated cavity. Contrary to what Buck states, I have occasionally found small pneumatic cells in the roof of the antrum, and opening into it. Further,

the lateral sinus does not always lie in a groove along the back of the mastoid cells; this, certainly, is the most common arrangement, but frequently the cells extend for a considerable distance behind the sinus, so that it is imbedded in a groove on the inner aspect of the cells. This is important, for to reach the sinus from the outside the cells should be first perforated. Regarding the absence of pneumatic cells, I was rather surprised at the frequency of solid mastoids; I think I have found even a larger percentage of them than Zuckerkandl, but he examined a much greater number of skulls.

As the radical operation for the treatment of mastoid disease is draining of the antrum by perforating from the exterior, I tried to find a point on the surface corresponding to the antrum internally. After examining my specimens I came to the conclusion that such a point would lie *immediately* behind the posterior margin of the bony meatus, and *immediately* beneath a line prolonging horizontally backwards its upper margin. At this point I sent a fine drill (diameter $\frac{1}{10}$-inch) straight in—horizontally and transversely; and out of a hundred specimens, in every one which had an antrum—that is, in ninety-nine—the drill entered that cavity, or, in a small minority of cases, the cells immediately below and behind the antrum. In the majority of cases, as just said, the antrum was opened; but even in those comparatively few in which the cavity itself was not reached the opening of the cells situated on the walls of the antrum was all but as satisfactory; for these cells communicate freely with it, and besides, in disease of the antrum, its cavity is much enlarged, and the adjacent cells, owing to absorption of the bony trabeculæ, are thrown into it. I was satisfied that this point marked on the surface the position of the antrum sufficiently well; still, in the majority of cases, it might be more fully entered by shifting the drill point an $\frac{1}{8}$ of an inch upwards; in some few specimens an aperture at such a point would open into the cranial cavity, owing to the occasional low level of the roof of the antrum, as shown in a previous table. Consequently I adopted the former point for the sake of safety, and I believe it to be thoroughly effective.

I next measured in these specimens the distance of the inner wall of the antrum—that is, the outer wall of the posterior part of the labyrinth, from the exterior of the bone, measuring through the drill hole. To do this the more effectively I removed the roof of the antrum. The following was the result of the measurements:—

Distance from inner wall of antrum to exterior of bone—

$\frac{9}{16}$	inch in	2	specimens.
$\frac{5}{8}$	"	19	"
$\frac{11}{16}$	"	36	"
$\frac{3}{4}$	"	34	"
$\frac{13}{16}$	"	4	"
$\frac{7}{8}$	"	4	"

This gives an average of a little under $\frac{3}{4}$ of an inch, with a minimum a trifle under $\frac{5}{8}$ and a maximum of $\frac{7}{8}$ of an inch.

This distance was sought in order to decide how far the drill or other instrument should be sent in when boring for the antrum; for if the perforation go too far the labyrinth may be injured. Buck says the antrum should be reached at a depth of $\frac{3}{8}$ths of an inch. With this my observations agree completely. I calculated that the distance would be $\frac{5}{8}$ of an inch—that is, only $\frac{1}{40}$ of an inch more than Buck's measurement. He further says his rule is never to force the instrument beyond $\frac{3}{4}$ of an inch, although Schwartze places the extreme limit at 1 inch (25 mm.). I believe the distance laid down by Buck to be the more judicious—namely, $\frac{3}{4}$ of an inch. I have little doubt that an instrument sent in for an inch would, in a considerable number of cases, cause injury to the labyrinth.

My plan in drilling my specimens was this:—I placed a mark on the drill $\frac{3}{4}$ of an inch from its point, then I bored at the spot which I had found to correspond to the antrum, and I always found that I had bored far enough by the time that the mark on the drill reached the surface of the bone. The sensations felt on boring were—first, the decided hard resistance due to the outer table; next, an irregular gritty resistance caused by the diploe; then the drill ran freely into a cavity and was stopped suddenly by contact with the inner wall of the antrum. I feel sure that there can be very little difficulty

in finding the antrum in a case of mastoid disease where its cavity is much enlarged, if the hole be made at the point on the surface mentioned above.

I would refer incidentally to the inaccurate descriptions given of the position of parts represented on the upper surface of the petrous portion of the temporal bone, even in some of the latest and best text-books of anatomy. The eminence corresponding to the superior semicircular canal is taken as a landmark, then it is usually said there will be found a depression external to it corresponding to the roof of the tympanum, and that the antrum lies posterior and external to this. This is not accurate—the depression corresponding to the tympanum will be found in front of, not outside the eminence of the superior semicircular canal; the roof of the tympanum, which is oblong, crosses the line of the canal in front like the top of the letter T, with this difference, that it does not cross it at right angles, but rather obliquely, making a smaller angle externally than internally; while the depression external to the semicircular canal corresponds not to the tympanum, as usually stated, but to the antrum; further, this depression is frequently replaced by a well-marked eminence produced by the development of pneumatic cells in the roof of the antrum. Sometimes the part is much depressed, particularly in cases of absence of pneumatic cells or solid mastoids.

Having satisfied myself that the point mentioned above, immediately behind the meatus, and immediately below the level of its extreme upper border, marked upon the surface the position of the antrum sufficiently well for all practical purposes, and having found the distance of the antrum from the surface, I next sought for a method of passing a useful-sized instrument into the antrum, guided by this point and the information previously obtained. I may say at once, that in the majority of cases a $\frac{1}{3}$-inch, or even a $\frac{1}{2}$-inch instrument could probably be passed into the antrum and mastoid cells if the margins of the hole made lay immediately behind the meatus, and extended up to the level of its extreme upper border. But then, on the other hand, there was a certain number of cases in which such an opening would lead into the lateral sinus.

Unfortunately I could not distinguish from an examination of the surface which was a dangerous and which a safe case for such an operation; consequently I turned my attention to finding some method of reaching the antrum with a fair sized instrument, even in the worst and most dangerous cases, without injury to the sinus, and without entering the cranium.

The point which I have spoken of as marking on the surface the position of the antrum lies within a triangle, bounded by two constantly varying and one rather fixed side. The latter is the posterior border of the meatus, the remaining sides of the triangle are an upper corresponding to the level of the roof of the antrum, that is, the upper surface of the petrous portion of the temporal, and a posterior side formed by the lateral sinus. In this triangle the instrument must be sent in.

The first side, formed by the back of the meatus, is constant, or nearly so, but it ought to be remembered, as already pointed out, that this wall of the meatus occasionally, though rarely, runs nearly straight in, instead of sloping forwards. The upper boundary corresponding to the roof of the antrum is very variable. Its height, measured from the upper wall of the meatus just internal to its orifice, has been given already in tabular form. On reference to that table it will be seen that the distance usually varies from $\frac{1}{8}$ to $\frac{1}{4}$ of an inch with an average of over $\frac{3}{16}$, but in 2 per cent. of skulls it falls to $\frac{1}{16}$ of an inch. Putting this in other words, in almost every case a trephine hole may be made as high as a point $\frac{1}{8}$ of an inch above the upper margin of the meatus without danger of opening into the cranium; but in a certain small proportion of cases such a hole would open the cranial cavity, and in order to avoid this the aperture should extend only up to the level of the extreme upper margin of the meatus, and should never reach higher than this.

It ought to be pointed out here that if only a shallow opening is being made not exceeding a depth of $\frac{1}{8}$ of an inch, or thereabout, it may be at a higher level than that just suggested; for, owing to the sloping upwards of the superior surface of the petrosa where it abuts against the squamous portion of the bone, there will be no danger of entering the cranium, unless the aperture be of some depth.

The most important of the boundaries of the triangle, and, I believe, the boundary at which there is the greatest danger, is the posterior, formed by the lateral sinus. It has been pointed out that the position of the sinus is most variable; it is sometimes near the meatus, sometimes far off, sometimes it lies near the surface of the bone, again it runs deeply. A closer examination of its relations and varieties will be useful.

We have already seen that the lateral sinus, after arching upwards and forwards from the region of the occipital protuberance, turns downwards and forwards from a point about $1\frac{1}{2}$ inches behind, and a little over $\frac{5}{8}$ of an inch above, the centre of the auditory meatus (measuring from the upper margin of the sinus). At this point, as previously shown, it may bend gradually or sharply, then it runs downwards and slightly forwards in front of the posterior margin of the mastoid—not quite parallel to it, but rather more vertically—and finally turns into the base of the skull at about $\frac{1}{4}$ of an inch below the level of the floor of the meatus. Its relation to the posterior margin of the mastoid I have already referred to, and I have pointed out how much this part of the sinus varied in its position. Its varieties in two special directions are of particular importance just now—namely, in its relations to the posterior wall of the meatus, and in its distance from the external surface of the bone. Regarding the former I must point out that, owing to the direction of the sinus forwards as well as downwards, and the curved form of the posterior wall of the meatus, the distance between the two will be greater above than at the level of the centre of the meatus. Consequently I have selected the latter level, and measured in a hundred specimens along the base line, that is, opposite the centre of the meatus, the distance of the anterior margin of the sinus from the posterior wall of the meatus—

It measures	$\frac{3}{16}$	of an inch	in 1	case.
"	$\frac{4}{16}$	"	1	"
"	$\frac{5}{16}$	"	4	cases.
"	$\frac{6}{16}$	"	20	"
"	$\frac{7}{16}$	"	17	"
"	$\frac{8}{16}$	"	17	"

It measures $\frac{9}{16}$ of an inch in 15 cases.
" $\frac{10}{16}$ " 18 "
" $\frac{11}{16}$ " 4 "
" $\frac{12}{16}$ " 3 "

This gives an average of about half an inch as the distance of the sinus from the back of the meatus, measured on the base line. But here, in particular, averages are of little practical value. What we must seek is the most anterior point at which the sinus may be found. On looking at the table it will be seen that in one case it was $\frac{3}{16}$ of an inch, in one $\frac{1}{4}$ of an inch, and in four $\frac{5}{16}$ of an inch from the meatus. We may put these together for practical purposes, and say that in 6 per cent. of cases the sinus will be little more than $\frac{1}{4}$ of an inch from the back of the meatus. Two of these 6 in my specimens were solid mastoids. In the majority of the specimens the distance between the sinus and the meatus ranged from $\frac{3}{8}$ to $\frac{3}{4}$ of an inch. If we are to regard the rights of such a small minority as 6 in 100, we must seek some means of avoiding the sinus in these dangerous cases. In considering how to do this in the 6 specimens referred to, it must be remembered that the distance of the sinus from the meatus given is that measured at the base line, and consequently the shortest. As the sinus slopes upwards and backwards, and the meatus upwards and forwards, the distance between the two is greater the higher up we go; but in seeking this wider interval, the line of the roof of the antrum must not be encroached upon if we are to avoid opening into the cranium. The line of safety in this direction has been already laid down; unfortunately this line is not at as high a level as would permit of the selection of a site corresponding to the widest interval between the sinus and meatus. In the 6 cases under consideration the triangle within which the boring should be made was reduced to a very small area, still in each of them a $\frac{1}{4}$ inch drill might, if properly placed, be sent in without injuring the sinus, and without encroaching on the line of the roof; in one of them, certainly, only with the greatest care—namely, that in which the sinus was only $\frac{3}{16}$ of an inch from the meatus. In the two worst of these cases the use of a larger instrument could not be thought of; it would

undoubtedly injure the sinus. In the other four, *possibly*, a $\frac{1}{3}$ inch drill might be used.

A consideration of these facts led me to the conclusion that, if we are to adopt some means of avoiding the sinus in these bad cases—which are about 6 per cent. of all examined—we must use a $\frac{1}{3}$-inch drill, or other boring instrument; and secondly, we must select a particular site which will be within the triangle of safety referred to above. This second point will be accomplished if we place the drill, the $\frac{1}{4}$-inch drill, so that the aperture it makes shall be *as near as possible* to the posterior border of the meatus, and its upper margin *quite as high as the extreme upper margin of the meatus.* In those cases in which the upper border of the meatus is ill-defined, and slopes gradually inwards, the point at which it first markedly begins to slope in should be taken as representing its upper margin. If the operator is willing to run the risk of opening into the cranial cavity in 2 per cent. of cases, he may, with greater safety to the lateral sinus, allow the upper margin of the drill-hole to reach to a height of one-twelfth of an inch above the border of the meatus. Even if the cranium be opened into slightly, I imagine few evil results would follow; the fact that the cavity was entered would soon be discovered, and care might be taken to go no further in that direction. If this danger of opening the cranial cavity in 2 per cent. of cases be not of sufficient gravity to give trouble, I would strongly recommend the adoption of the latter level, namely, that in which the upper margin of the drill-hole is at a level of one-twelfth of an inch above the border of the meatus.

So much for the point on the surface at which the drill should be applied; now as to the direction in which it ought to be driven in. I believe it will be most judicious to pass it straight in—horizontally and transversely—neither forwards nor backwards, upwards or downwards. Certainly if it be directed a little forwards there will be less danger of wounding the sinus, still I think this is counterbalanced by three considerations—first, the posterior wall of the meatus occasionally slopes very little forward; secondly, there is danger of wounding the facial nerve, which runs down along the inner margin of the opening from the tympanum into the

antrum, if the drill be directed much forwards; and thirdly, the aperture may be made in safety without resorting to this means of avoiding the sinus.

Summing up, then, it is my experience that a $\frac{1}{4}$-inch trephine or drill may be passed directly in at the spot indicated above without any great danger. Such an opening will drain the antrum, and if it be considered of insufficient size it may be carefully enlarged at its superficial part—for near the surface the aperture may be made very large without danger—and this enlargement continued inwards if no difficulty be experienced. If a larger drill be selected there will be undoubtedly danger in a certain, but small number of cases. But under all circumstances the anterior and upper borders of the opening made ought to be at the lines indicated above.

I must here deprecate strongly, from an anatomical point of view, the site given by some authors for opening the antrum—namely, a quarter (or by others a half) inch behind the meatus, and below the level of its upper border.

I have referred incidentally to the facial nerve, and I have pointed out that it will not be in danger when the drill is sent straight in at the spot selected; it will lie in front of such an aperture. But if, on the contrary, the instrument be directed forwards, the nerve will be in danger if the drill be passed far in.

Another important relation alluded to previously is the distance of the lateral sinus from the exterior of the bone. This is evidently a matter to be considered in connection with opening of the mastoid cells behind the meatus.

I have measured the distance of the sinus from the surface in 90 cases with these results:—It was $\frac{1}{12}$th of an inch in 1 case, $\frac{1}{8}$th of an inch in 2, $\frac{3}{16}$th in 9, a $\frac{1}{4}$-inch in 21, and in the remaining cases it varied from a $\frac{1}{4}$ up to $\frac{19}{16}$th of an inch, with an average of about $\frac{5}{16}$th.

The cases in which the sinus was very near the surface were those in which it lay entirely behind the mastoid cells, so that it was separated from the surface simply by the diploic and outer and inner tables of the skull, or sometimes merely by a layer of compact bone.

These figures show how dangerous indiscriminate trephining behind the ear may be; in 1 case a hole only $\frac{1}{12}$th of an inch in depth would expose the lateral sinus, a hole $\frac{1}{8}$th of an inch would accomplish this in 2, and it required only a hole $\frac{3}{16}$th of an inch in depth to expose it in the other 9. I think the practical inference from a consideration of these facts is that all apertures behind the meatus should be made with the greatest care, for we know not how near the lateral sinus is.

What instrument should be used is a question upon which I can hardly offer even a suggestion, but I know it ought to be an instrument the progress of which can be seen and kept under control. Further, the sinus should be expected at every movement, and no bold cutting ought to be done; then if the sinus be exposed, with such precautions, no harm in all probability will result from the mere exposure.

After this long digression I return to the starting-point of my communication, namely, the anatomy of Mr. Wheeler's operation. In his paper on "Trephining in Mastoid and Tympanic Disease,"[a] p. 9, he says, "The site I would select for operation, . . . would be such as to place the lower border of the trephine on a level with the external auditory meatus, and anterior to a line dividing vertically the mastoid process. By adopting this course there will be no danger of wounding the lateral sinus, the tympanum and mastoid cells will be opened, giving full exit for discharge, the dura mater will be exposed, and should pus exist between it and the cranium, there will be ample freedom for its escape."

What exactly Mr. Wheeler wished to convey in the foregoing it is not easy to say. The lower border of the trephine is to be placed on a level with the external auditory meatus; we are not told what part of the meatus—upper or lower border, or centre. Again, the trephine is to be placed in front of a line dividing vertically the mastoid process; such a line may be near to, or far from, the meatus, for the process may be divided vertically at any part of its surface. But reading Mr. Wheeler's words in the most favourable way, I presume he wished to convey that the lower

[a] Fannin & Co., Dublin.

margin of the trephine crown should be placed at the level of the upper border of the meatus, and the crown itself in front of a vertical line running through the most prominent inferior point or tip of the mastoid process (or possibly a vertical line dividing the process as a whole into two equal parts). If a trephine hole be made according to these directions, as a rule the lower part of the opening will correspond to the base of the petrous portion of the temporal bone here applied to the squamous, and the upper part, to the dura mater of the middle fossa of the skull. As Mr. Wheeler says, there will be no danger of wounding the lateral sinus; the dura mater and mastoid cells will be exposed; but the tympanum (or antrum) could not possibly be laid open by such a trephine-hole alone, without great injury to the brain, for the tympanum is three-quarters of an inch from the surface, and if the lower part of the trephine were sent in to open this cavity, the upper part would burrow for a corresponding distance into the cerebrum. If, on the other hand, the crown of bone be removed when the upper part of the trephine has pierced the thin squamous plate some distance above the meatus, there will be exposed in the lower part of the opening, not the antrum or tympanic cavity, but simply the superficial mastoid cells—at least this would be the case in the normal mastoid; possibly in a case of disease the interior might be so much eroded that the antrum reached practically to the surface.

In a normal case, after removing the crown of bone, to reach the antrum it would be necessary to drill a hole straight in at the lower and back part of the trephine opening. Occasionally, owing to the low level of the superior surface of the petrous, as shown previously, a trephine sent in according to these directions would expose little more than the dura mater, with possibly just a very thin area of the petrous along the lower edge of the hole.

Certainly the operation gives free scope for relieving intra-cranial pressure from pus, the temporo-sphenoidal lobe may be explored directly, and the cerebellum by a curved needle, &c., through the tentorium. But there are two objections to it—the mastoid cells, when the upper surface of petrous lies low, may not be exposed at all, or only in a very slight degree; and secondly,

there may be considerable difficulty in removing the piece of bone when it is connected strongly to a petrous which ascends higher than usual.

Probably a more practical line than that which bisects the mastoid process would be a vertical line $\frac{3}{8}$th of an inch from the posterior margin of the meatus. Owing to the varieties of the shape of the mastoid the former line is by no means constant, and as the varieties in the shape of the process bear no fixed relation to the varieties in the position of the sinus, it would be better to take some safe fixed line, such as that proposed.

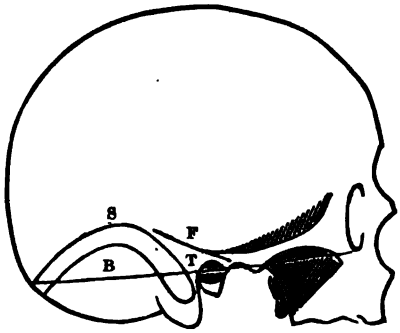

Fig. 1.

S, lateral sinus; B, Reids's base line; F, level of floor of middle fossa of skull; T, triangle in which drill-hole may be made. It is small in this case; the sinus arches up very high, and comes far forward. Observe marked curve of sinus and its relation to base line (from a photo).

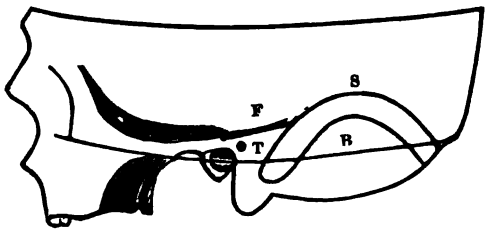

Fig. 2.

S, lateral sinus; B, Reid's base line; F, floor of middle fossa; T, triangle for drill-hole. The triangle is large in this case; lateral sinus considerable distance from meatus; F is rather low. Observe relation of sinus to base line. The round mark in front of T is the point corresponding to the antrum (from a photo).

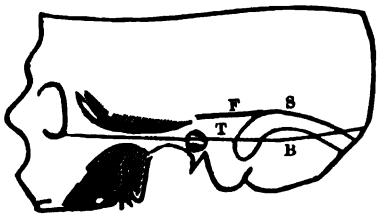

Fig. 3.

S, lateral sinus; B, Reid's base line; F, floor of middle fossa; T, Triangle for drill-hole; triangle large. This is the only case in which the lateral sinus lies near the base line (from a photo).

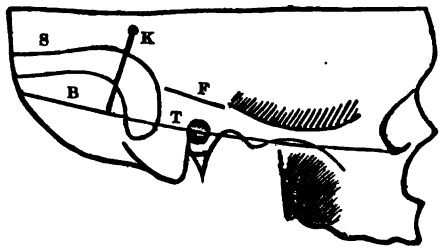

FIG. 4.

S, lateral sinus; B, Reid's base line; F, floor of middle fossa; T, triangle for drill-hole; K, site proposed by Mr. Barker for trephining in temporo-sphenoidal abscess. Observe the proximity of the lateral sinus in this case to the site. Here the lateral sinus lies high above base line posteriorly, and it is enlarged where it bends down on the mastoid (from a photo).

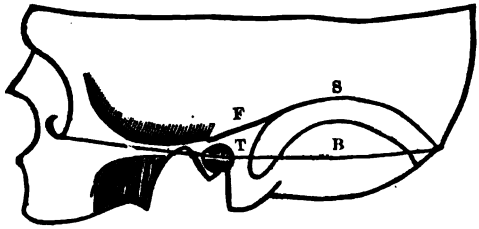

FIG. 5.

S, sinus; B, base line; F, floor of middle fossa; T, triangle for drill-hole. Here the sinus is rather near the meatus, and F is low (from a photo).

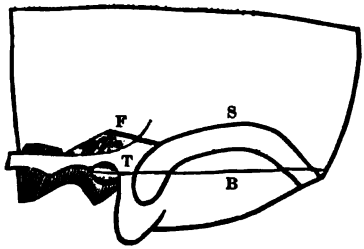

FIG. 6.

S, sinus; B, base line; F, floor of middle fossa; T, triangle for drill-hole. In this case the sinus presents abrupt bends instead of gentle curves, and it approaches near to the base line, and near to the meatus (from a photo).

A CASE OF CHOLECYSTOTOMY.

By T. MYLES, M.D., F.R.C.S.;
Surgeon, Jervis-street Hospital;
Professor of Pathology, Royal College of Surgeons in Ireland.

[Read in the Section of Surgery, May 9, 1890.]

The operation of cholecystotomy is as yet sufficiently infrequent in Dublin to warrant me in bringing the present case before the notice of this section of the Academy.

Although a considerable number of cases are reported by various English surgeons, still, for some reason not very plain, but few cases—less than half a dozen I should think—have been reported from Ireland.

In the majority of cases reported, as in mine, the operation has been performed for accumulations of biliary calculi. The fact that this condition is usually associated with high feeding and good living—a combination unhappily too rare in Ireland—may explain our apparent immunity from biliary disorders severe enough to demand operative interference.

The case which I have the honour now to bring before you is simple enough, and will not make any great demands upon your patience:—

Case.—Mrs. O'C., a stout dame, of some sixty summers, was admitted into Jervis-street Hospital in the month of November last, under the care of my friend and colleague, Dr. William Martin.

She complained of intense agonising pain in the abdomen, not limited to any particular area, but most marked apparently beneath the xyphoid cartilage, between it and the umbilicus. When asked to describe the character of the pain she stated that it was as if her intestines were obstructed. She had no pain in the right shoulder or right arm. In addition to this frightful pain she had also incessant vomiting and straining; her bowels were obstinately

constipated, not having been moved for more than a week; she was deeply jaundiced; her urine was of a porter colour; her tongue was thickly coated; and she was extremely weak and prostrate. Her temperature was never higher than 101°, and her pulse, in spite of the jaundice, was over 80 beats per minute, the usual slowing of the pulse found in such cases being entirely absent.

On examining the abdomen we found that it was acutely distended, tympanitic, and tender; the tenderness was most marked at the right of the umbilicus, below the costal cartilages. On careful palpation a small rounded tumour could be detected in this situation, but it was extremely difficult, owing to the great tenderness, to make a satisfactory examination.

My colleague, Dr. Martin, diagnosticated the case as one of impacted calculus in the bile duct, with subsequent distension of the gall-bladder, and at once suggested operative interference. After anxious consultation we decided to wait a little longer, and meanwhile try the effects of copious enemata and morphia.

The enemata, at the third attempt, brought away a copious stool, hard, clay-coloured, and fœtid; the tympany subsided, and the pain abated somewhat.

This relief, unfortunately, was only temporary, and on waking from a too short sleep produced by large doses of morphia, she developed all her former symptoms in an aggravated form. Her condition now was truly distressing; the violent spasms of pain appeared to paralyse her vital energies; she lay on her side doubled up in bed, continually making futile efforts to vomit; her eyes were sunken, her extremities deadly cold, her pulse was barely perceptible. She besought us to do anything to end her frightful sufferings, and when, after a second consultation, it was decided to perform the operation, which my colleague, Dr. Martin, had been strenuously urging throughout, I confess I felt a twinge of horror as I looked at the frightful collapse of the patient, and thought that through lack of boldness on my part I had allowed the too valuable time to pass away, and through a caution that was almost cowardice had postponed operation until a time at which recovery from severe shock was almost impossible.

However, it was now obvious that if something were not done at once our patient had but a few hours to live, and I determined to take the risk, whatever it was, and make one effort at least to relieve her dreadful sufferings.

Chloroform having been very cautiously administered, the rigidity

of the abdominal wall gave place to a flaccid condition, and we were now able with ease to make out a distinct rounded tumour behind the right rectus muscle, a little above the umbilicus. It measured apparently about two inches in diameter, and was perfectly smooth. The skin and abdominal wall moved freely over it. The tumour was dull on percussion, immobile in a vertical direction on pressure above or below, but slightly mobile from side to side. It moved slightly with the respiratory movements. The diagnosis now was much clearer, and everything being ready, with the kind assistance of the President and my colleagues, I made an incision through the skin exactly over the tumour about three inches long, parallel to the linea alba, and about one inch to its right side. Going boldly through the fibres of the right rectus muscle, I struck across the anastomotic arch between the interior mammary and epigastric, without wounding it, fortunately. The artery was tied in two places and divided between the ligatures. Continuing the dissection, the posterior sheath of the rectus was exposed and carefully opened, there being no hæmorrhage. The peritoneum was carefully cut into, and to my intense delight the gall bladder sprang into the wound—yellow, tense, smooth, and elastic. Hot sponges were now packed into the wound, and a large one over the gall bladder.

Putting my finger and thumb into the wound I grasped firmly the neck of the bladder and steadied it. I now thrust a fine aspirator needle into it, and drew off a considerable quantity of perfectly clear mucus. The tumour now collapsed, and the calculi could be distinctly felt through its wall. Still steadying the bladder with my left index finger and thumb I cut into its summit freely enough to admit nearly two fingers. Putting my finger into the gall bladder I began to turn out the calculi, which I have now the honour to exhibit to you. They number twenty-five.

The President of the College very kindly kept the tally until the number reached twenty, and then he gave it up. The bladder being now empty, I determined to explore the duct.

Having secured the edges of the gall bladder by a pair of clip forceps, I gave them to my colleagues, Messrs. Dwyer and Cranny, and urged them to keep the bladder drawn well forward through the wound, and thus in a slight degree to straighten out the duct. I now removed the sponges from the abdomen, and, first warming my hand by dipping it in warm water for a few minutes, I passed my entire hand through the abdominal wound. Running my fingers carefully along the cystic duct, I could find nothing. I

then passed my index finger into the foramen of Winslow, and began to explore the common duct from above downwards. Just at the extreme limit to which I could reach, behind the first part of the duodenum, I felt another calculus. For a while I made ineffectual efforts to dislodge it by pressure from behind. At length I determined to try to use both index fingers, and with my left in front, steadying the first part of the duodenum, I made vigorous pressure from below upwards on the posterior aspect of the duct with my right. After a few minutes I felt the stone yielding, and then to my great joy it moved rapidly upwards and was easily coaxed along the cystic duct into the gall bladder. Another careful exploration was made, but nothing further could be discovered.

Hot sponges were now again packed into the wound, and I hastened to complete the operation.

Grasping the cut surface of the parietal peritoneum, I carefully sutured it to the edges of the skin wound with a number of interrupted silk sutures; the wound in the gall bladder was temporarily plugged with a small sponge, and the nozzle of the irrigator thrust far down into the abdomen. A stream of warm boracic lotion—temp. 100° F.—was now sent through the abdominal cavity and allowed to flow continuously for several minutes; this seemed to have a very stimulating effect on the respirations, which became deep and regular. The lower part of the wound was now closed by a double row of sutures, the deeper through peritoneum alone, the superficial through everything except peritoneum. To the open upper end of the wound the cut edges of the gall-bladder were fixed, so as to make a fistulous opening into the bladder; a drainage-tube was now fixed in that viscus, and the entire wound freely dusted with iodoform. A large pad of antiseptic wool and abdominal bandage finished the dressing, and the operation was complete.

She was now taken back to bed, still very collapsed, but on recovering from the chloroform she stated that she was nearly free from pain. A hypodermic injection of morphia was now given, and hot bottles packed around her. Her temperature after the operation was 97°, and her pulse so weak that I could not estimate its rapidity.

Teaspoonfuls of champagne containing a little brandy were given her, and hot flannels placed over the cardiac area.

On the following morning she had rallied somewhat, but was still very weak.

I will not weary you with details of her condition and treatment from day to day, but will merely speak of what I believe will be of general interest and utility.

For four days after the operation the urine still remained porter-coloured, and the jaundice deep. After that time the colour began to fade, and we looked anxiously for a movement from the bowels. On the 5th day, as there was still no movement, a large soap and water enema was administered, but it failed to produce any result. We then discovered that the rectum was packed with hardened fæces, necessitating removal with fingers and scoop. After this an enema produced a copious stool, with great pain and tendency to collapse, but the stool was still completely devoid of bile. Two days after this repeated small doses of magnesium sulphate brought about a good stool containing bile. From that date the bile continued to increase in the stools and disappear from the urine.

Throughout all this period bile was escaping from the wound freely; at first I merely used a short drainage-tube, but as I found that the fluid soaked through the dressings, I substituted for it a long tube with a short piece of bent glass tubing as a connecting link—one end was of course in the gall bladder, kept in position by a transverse piece of adhesive plaster; the other was in a basin by the bedside, and into this basin all the bile flowed and could be collected.

I may mention here what is, I think, worthy of notice—namely, that during the first four days, while the bile was flowing freely from the wound, the urine was still dark-coloured, and contained bile in abundance. It is perhaps possible that this bile in the urine was only what had previously been absorbed into the blood, and which now took four days to be eliminated. It is, on the other hand, impossible to believe that after the operation, with a large open wound in the gall-bladder, any appreciable quantity of bile could have been absorbed by the hepatic capillaries.

The wound healed rapidly, and after the 14th day, with abundance of bile in the stools, I removed the drainage-tube and allowed the fistula to close. When everything seemed at its fairest, however, she got an attack of gastritis, the result of well-meant, but dangerous, kindness on the part of her relations, who smuggled into the hospital a quantity of indigestible food, which she greedily devoured.

The bile again burst from the wound and disappeared from the

stools; jaundice again appeared, and she seemed in a fair way to relapse into a condition but little better than that from which she had just been rescued.

A large dose of calomel, followed by the rose mixture of the hospital, and three days' starvation diet, however, brought relief, and from that on she progressed, without a single bad symptom, until she finally left the hospital five weeks after the operation with the wound *completely* closed and perfectly free from all trace of the terrible illness through which she had passed.

I regret to say that no accurate record of the amount of the bile passed daily through the tube was kept. My idea is that the average quantity varied from 10 to 15 ounces. Professor Purser tells me that recent investigations show that the amount of bile secreted daily may be estimated at 4 oz. per 14 lbs. of body weight. On this calculation my patient ought to have voided 50 oz. of bile per diem. Although I do not know exactly the amount she passed, I am quite certain that half this amount would more than represent the quantity of bile she discharged daily through the tube.

Another point of interest is the colour of the bile, the beautiful golden yellow described by writers on Physiology was conspicuous only by its absence, and in its place a very dark olive-green coloured fluid was to be seen.

Evidently bilirubin does not exist in fresh bile, but is produced from biliverdin by reduction in the intestine.

Before closing my paper I would like to say a few words about the operation itself, its advisability and technique.

That the operation is justifiable few will deny; the result of an ever widening experience by surgeons in all countries has proved beyond doubt that in appropriate cases it offers not merely a means of prolonging an invalid existence, but of restoring a patient to full vigour and active health.

The real crux of the question is—Can we guarantee against a permanent biliary fistula resulting from the operation? It has been asserted by distinguished surgeons that such a result is compatible with health and longevity, and that all our previously held

views as to the functions of the bile must be erroneous; that, in fact, instead of a useful fluid it is entirely excrementitious.

With this view, however, attractive though it be by its boldness and utter disregard for clinical and experimental experience, I cannot quite agree. The experience of commonplace everyday practitioners is that obstructive jaundice is invariably associated with constipation, wasting, and fatty stools. Experimental physiology shows us that bile plays an important *role* in the digestion of fats, and the fact that the bile duct in almost all animals opens in almost the same situation is another proof of its universal importance and utility. No, I would say that the possibility of a permanent fistula is a dark shadow over the operation, and however we may gloss it, it must be regarded as a disaster; and an operation with such a finale can only be regarded as in a minor degree a success.

The next question I would ask is—Ought the wound to be closed at once, and should we trust to the bile finding its way along the cleared channel into the intestine? To this I would answer in the negative. We ought not to close the wound at once, and my reasons for saying this are twofold—1st. We can never be absolutely sure that the entire channel is cleared; the narrowest part of the duct, where a stone is most likely to lodge, is the most inaccessible—namely, that part which lies between the pancreas and duodenum. A stone lodged here could not be felt in the first place, and I doubt, if even it were felt, that it could be removed by any methods yet devised, except by the very unreliable one of injecting fluids capable of dissolving the concretion.

Again, even though all the concretions be removed from the channel, the wall of the duct must necessarily be paretic for some days afterwards, and incapable of propelling the fluid against the resistance offered by the obliquely-penetrated wall of the duodenum, and thus bile would still accumulate above the obstruction, and the phenomena for which the operation had been undertaken would recur with their original virulence.

Finally, in the modified operation it has been clearly proved that

when the channel is free the external wound heals without any difficulty.

Before concluding my paper, I would wish again to say that the credit of the accurate diagnosis belongs entirely to my colleague, Dr. Martin, and that his urgent solicitations and earnest support, together with the kind and valuable co-operation of my colleagues, encouraged me to undertake an operation hazardous in the highest degree, but happily carried to a successful issue. My thanks are also due to our two House Surgeons, Drs. Mills and Hughes, and to my pupils, Mr. (now Dr.) Kearney and Mr. Ryan, for the unremitting care and attention they bestowed upon my patient during the anxious days that followed the operation.

A CONTRIBUTION TO THE SURGERY OF THE BRAIN.

By WILLIAM THORNLEY STOKER, Surgeon; and GUY L'ESTRANGE NUGENT and JOSEPH O'CARROLL, Physicians;

Richmond, Whitworth, and Hardwicke Hospitals.

[Read in the Section of Surgery, May 9, 1890.]

THE two cases about to be recorded are ones respectively of tumour and of abscess in the cerebrum. Although operation in the former yielded but temporary benefit, yet it is of scarcely less interest than the latter, in which operation was attended by complete success. Both have a bearing on the diagnosis, localisation and treatment of the classes of complaints to which they belong. Our failures in surgery are generally more instructive than our successes, for calamity is a more direct guide to danger than is success. One is positive, the other negative, in its value. The limited measure of success, therefore, which has attended one of these cases offers the stronger reason why they should both be recorded.

The first case is one of tumour in the superior parietal lobule on the right side, which was diagnosed and operated on without success, the relief being but temporary, and the patient dying three weeks after operation.

The second case is one of abscess in the right temporal lobe, which was diagnosed and operated on, and the subject of which is now alive and doing well.

CASE I.—J. B., aged forty-two, a boiler riveter, was admitted into the Richmond Hospital on December 12th, 1888. He was of muscular, vigorous habit, temperate, had not had syphilis, and, until the day of his admission, suffered from no blemish except slight deafness, produced by his noisy occupation. He was a

widower with two children, and a man of quiet, regular habits. His father died of phthisis, his mother, probably, of malignant disease of the stomach.

On December 12th he left work at 5 o'clock, p.m., feeling perfectly well. When walking home the lace of his boot became loose; he stooped to fasten it, and when raising himself became giddy. He recovered himself in a few minutes, but his left leg began to jerk, the movements commencing at the instep and gradually extending up the extremity, and engaging the same side of the trunk. He was unable to walk, had to catch the railings to prevent himself from falling, and feared that the passers-by would think he was drunk. He was assisted home, and brought to hospital the same evening.

When admitted he was, and had been, perfectly conscious and intelligent. He was sweating profusely; his pupils were dilated and symmetrical. His left leg was violently jerked by clonic spasms, occurring from 30 to 40 times in the minute, and engaging the extensor muscles on the front of the leg and thigh, and the adductors of the thigh. The limb was extended and rotated outwards. The left side of his trunk was engaged in the spasms to such an extent that he described them as "taking his breath away." The muscles about the left shoulder were affected, but to a much slighter degree. His face was turned to the *right* side. There was complete loss of sensation over the affected parts on the left side. He complained of much pain during the spasms. The knee-jerk was exaggerated on the left side.

His bowels were emptied by an enema, and he was put on large doses of bromide of potassium. After a few hours he was greatly relieved, and the spasms confined themselves to the leg.

December 13th.—Spasms are confined to the muscles on the front of the leg; sensation is completely recovered except on the front of the leg, where it remains slightly impaired. He is distressed by pains in the front of the leg and dorsum of foot; face slightly contracted on the *right* side.

From the time of his admission on December 12th his condition was one of steady improvement. His treatment consisted mainly in frictions to the left lower extremity, with a diluted ointment of atropia to relieve his severe pain; in the use of bromide of potassium, and attention to his bowels. His pain lessened; the spasms gradually disappeared in order from above downwards—the shoulder being the first, and lower extremities the last, to recover. The

pain and spasm remained in the leg after the thigh had recovered, then in the dorsum of the foot, and lastly in the great toe. He had no ocular symptoms, no headache, but his speech was, probably, a little more slow than it should be.

On December 16th he was able to leave his bed, but could not walk; on December 20th he was able to walk, and on January 8th he insisted on leaving the hospital and resuming his work, although we were anxious to keep him under observation.

The opinion formed of his case at the time was that he had probably a tumour near, or in, the leg centre in the right hemisphere; but the absence of ocular symptoms, of vomiting, and of headache rendered the diagnosis uncertain.

He remained well and at work until April 21st, 1889, when, as he was standing in the street, he was seized by violent clonic spasms of the muscles of the entire of the left side, except those of the face. The attack lasted for an hour, and then passed away. Two hours afterwards he got a second attack, even more violent than the first, and was brought to hospital. His condition was almost similar to that when under observation the previous December, but more pronounced. The differences were that his head was drawn to the left side, and he had severe frontal headache. He did not sleep for two days after his admission, and then only as a result of the use of bromide of potassium.

On April 30th, seven days after his admission, he was able to get up, the spasms having diminished in frequency and in degree, and being at this date only present a few times in the day, and confined to the leg. His temperature, when admitted, on April 21st, was 101°; it remained without much rise or fall, between 99° and 100°, until the 28th, when it became normal, and remained so. He was removed to the Whitworth Hospital, under the care of Dr. Nugent, on May 6th, his condition being then as follows:—

His left lower extremity was weak, but he could stand and move with the aid of a chair. He could not move the toes of the left foot. He had numbness of the leg; the plantar reflex was present; the knee-jerk of the left leg was exaggerated. He could flex the leg, but could not retain it in the flexed position. He suffered from giddiness, which disappeared when he had been out of bed for an hour or so.

This state of affairs lasted with little ups and downs, his condition on the whole being an improving one, until May 20th, the

only marked inconvenience he suffered being from a severe pain on the inside of the thigh. On May 20th he got another attack of spasm. The movements commenced in the knee and worked up the left side to the head. He complained of great pain over the stomach, and of severe frontal headache; had twitchings of the extensor muscles of the thigh, of the abdominal muscles, the pectorals, and trapezius of left side. No twitchings of arm, face, or tongue.

May 23rd.—Better; twitchings have ceased in trapezius and pectorals; muscles still affected are adductor longus, tensor vaginæ femoris, rectus abdominis, external intercostals—all of left side. Could walk across the ward without assistance; left side weak; he brings muscles of right side into play, and swings left limb in the arc of a circle.

May 24th.—Is much better; movements cease during sleep; had slight clonic spasm of the adductor longus, rectus abdominis, external oblique, external intercostals, and scalenus anticus of left side. The spasms occur about twenty times in the minute; they disappeared in order from above downward, the adductor longus being the last to recover.

From May 24th to June 13th he continued in much the same state—able to be up for the greater part of the day, sleeping well at night, suffering from numbness on the front and outside of leg, occasional giddiness and spasms at times, of a mild character, and of short duration. The spasms always commenced in the foot, extended to the thigh, came on suddenly, were attended by pain and perspiration, lasted from five to ten minutes, and were followed by weakness and numbness in the parts.

June 13th.—Had an attack of spasm in the left leg, which lasted, at intervals, for two hours.

June 20th.—Had an attack of clonic spasm, affecting only the left arm, deltoid, triceps, and trapezius muscles, which lasted off and on until July 2nd. Through July he remained in much the same state, and, on August 1st, had a severe attack of spasm. His arm was jerked at a rapid rate from the elbow up, his head turned to the left side, his eyes open, and he could not speak for several minutes. The attack lasted for over an hour, and was followed by loss of power in the hand and arm, and by severe and constant pain in the shoulder.

August 8th.—Spasms, chiefly engaging the trapezius, deltoid, biceps, and supinator longus.

August 10th.—The loss of power in the hand persisted; all the muscles of the upper extremity responded to electrical stimulus.

August 15th.—The left side of the face was paralysed.

August 19th.—Intellectual powers are failing; the patient does not know what he is speaking about; he cannot return an intelligent answer. His tongue is normal, and protruded straight. He has, for the first time, complete loss of sensation in the arm.

From August 19th his condition was one of rapid deterioration. His patellar reflex was still exaggerated on the 21st. His eyes were examined by Mr. Swanzy on the 23rd, who found no optic neuritis, or evidence of disease. On the 28th he passed urine unconsciously, but had some power of giving notice when the bowels were about to act. He was in a state of semi-consciousness, lying quite helpless, and dead to his surroundings, unless roused. When roused, and asked if he had pain, he put his hand, slowly, to the right side of his head and gave a stupid assent. When loudly and repeatedly desired to put out his tongue, he did so slowly, but in the case of each movement did not attempt to recover himself, leaving his hand at his head in the one case, and allowing his tongue to remain protruded in the other. He swallowed slowly and with difficulty. His temperature was normal, 98·4°; his pulse and heart weak.

The next morning, the 29th of August, he was so much worse that it was evident he would soon die if operation were not undertaken at once, and so we proceeded to trephine the skull and explore the brain.

Previously to operation he was unable to speak, could not swallow, was completely paralysed, both as to motion and sensation on the left side, including the face; his pupils were dilated and symmetrical; he had nystagmus; and could be roused only by great effort to make any response, his extreme of intelligence being to put his right hand to his head when loudly questioned as to the existence of pain; he retained his hand at his head until it was removed from it.

Operation.—As the leg centre was pointed to by the clinical history of the case as the starting point of the disease, its situation was selected for operation. The flap of scalp was cut in a semicircular fashion, with its convexity corresponding to where the inner edge of the trephine hole would be—the advantages of forming the flap in this fashion being, that it leaves its chief vascular and nervous supplies, which are from below, unimpaired, and that

the size of the flap can be indefinitely extended by carrying the incisions downwards towards the ear in case additional apertures in the skull are required. The trephine, 1 inch in diameter, was first applied over the upper part of the fissure of Rolando, as near to its upper end as the safety of the longitudinal sinus would allow. A second trephine opening was made, antero-inferior to the first, in the line of the fissure of Rolando, and the angles of bone being removed with a forceps, an oval opening, 1 inch in diameter and 2 inches in length, was obtained. When the dura mater was incised the brain bulged in the characteristic fashion seen in cases of great intracranial pressure. No evidence of diseased structure could be seen in the exposed area of brain. The region of the leg centre was then explored both with a probe and a trocar, and as nothing evidencing disease was found the entire motor area was then similarly examined, but without result. The intracranial pressure was so pronounced that when a silver probe was introduced into the brain substance, and left there, it was rapidly extruded by the force exercised by the brain.

It became evident that no good could result from further exploration; so the membranes were replaced, the flap adjusted by points of suture, a rubber drainage-tube passed across its base, and an antiseptic compress applied over all. I did not replace the bone, because it was now evident that the operation could give even temporary relief only by allowing some degree of protrusion so as to relieve the intracranial pressure.

His temperature, which had previously been normal, rose on the evening of operation to 101°, and the next day, August 30th, to 103·5°. It then fell so as to become normal on the third day after operation, and remained so until August 17th, when it began to rise, and by the day of his death, September 19th, had risen to 105·4° It was essentially the same on both sides all through the case.

August 30th.—Speech and intelligence improved; some tonic spasm of right side of face; requires to have a catheter passed; nystagmus continues.

August 31st.—Speech and intelligence much better; passes water easily; takes nourishment freely; complains of great pain in the head; can open and close both eyes symmetrically when the tonic spasm of right side of face is absent.

September 1st.—Drainage tube removed; general condition improved; power returning in left arm. The wound has all

united, except the apertures where the drainage tube passed through. The surface of flap raised above surrounding skin by pressure of the brain.

September 2nd.—Tonic spasm of *left* side of face.

His condition from September 2nd to September 8th was one of improvement. He was fairly intelligent, had improving power over the left arm, and continued to have an oozing of softened brain substance through the holes where the drainage tube had been; the relief from pressure thus obtained being the cause of his temporary improvement. On the night of September 8th he was restless and semi-delirous, tore off his dressings, and opened the flap, so that next morning we found a large hernia cerebri. He was, so far as mental condition and paralysis were concerned, in no worse condition than before this accident. From this date, however, he began steadily to disimprove. A large destruction of the protruding brain and, as *post-mortem* examination afterwards showed, of the neighbouring substance took place by suppuration and sloughing in spite of every antiseptic measure we could use. The condition of paralysis of the left side from which he had partially recovered, so far as his face, trunk, and upper extremity were concerned, returned; and, on September 18th, he became completely hemiplegic, passed into stupor, finally into coma, developed hiccough, cerebral vomiting, preagonistic temperature, and died on the following day, September 19th.

Post-mortem, September 21st.—The hernia cerebri formed a protrusion about 3 inches in diameter and 1 inch beyond the level of the skull. The calvarium separated from the dura mater with normal readiness. Around the trephine opening the membranes were adherent to each other, so as to seal the cavity of the arachnoid. There was pronounced arachnitis about the base of the brain—most marked under the cerebellum, the medulla oblongata, and the anterior lobes of both sides, but more extensive on the right side, where the inflammation was not merely basic but extended up on the frontal convolutions.

Corresponding to the trephine opening at the fissure of Rolando, and in the centre of the hernial mass, was a ragged, sloughing, suppurating cavity, extending for more than an inch into the brain substance, but not engaging the ventricular cavity. On section, a tumour was found lying immediately behind this cavity, and occupying the posterior portion of the parietal lobe. It approached within half an inch of the longitudinal fissure, and within a quarter

of an inch of the surface of the cerebrum. It was spherical in form, about one inch in diameter, and of equal consistence and resistance to normal brain matter.

The tumour has been examined by Professor Purser and Dr. Conolly Norman. It is a spindle-celled sarcoma.

In addition to the value which in the present unripe state of our knowledge of brain tumours attaches to every case which can add to clinical or statistical information, there appear to be some points of special interest in this record. As regards the clonic spasms, although intermission in their occurrence is the rule, the intermissions here were of an unusual character. They were irregularly extended over a very long period, and showed ups and downs of intensity instead of the progressive gravity which is generally seen. Besides, although they commenced in the leg and indicated the vicinity of the upper end of the fissure of Rolando as the focus of disease, and although these leg symptoms were followed by arm phenomena, and finally by engagement of the face—the natural sequence where a growth commencing in the centre for the leg gradually involved the centres for the upper extremity and face in the natural order of their anatomical relation—yet it was clinically and physiologically discrepant to find the arm or muscles of the trunk sometimes in a state of spasm, while the lower extremity, although not paralysed, was at rest. It is additionally out of rule to find that *post mortem* evidence discovered the tumour behind the situation where it might reasonably be expected to lie. The lessons derived from this part of the case appear to be:—

1. That the leg-centre extends further back than is usually taught into that part of the parietal lobule where its extension has been regarded as doubtful.

2. That the centre for the leg muscles is behind that for those of the thigh, as in this case the earliest muscles affected were those below the knee, and we may fairly conclude that the disease first engaged the part of the motor area next itself.

3. That what may be called referred pressure—that is to say, pressure remotely exercised—is a powerful and confusing factor in

the examination of brain tumour. For here it is evident not only that the small localised growth on the back of the leg-centre proceeded to engage the centres for the arm and face, but, what is even more remarkable, that it was able to affect them at times when its pressure spared and seemed to skip the intermediate and more closely related centre for the lower extremity.

The next point of great interest seems to be the absence of three out of that group of four symptoms which we are in the habit of relying on generally as indicative of tumour of the brain—namely, hemi-spasm, vomiting, optic neuritis, and fixed headache. Of these only the first was present; there was no optic neuritis, no vomiting of a cerebral character, and headache was a late, an occasional, and inconstant symptom.

It was this hiatus in the clinical history that caused so long a delay in adopting operation. This delay having occurred, it is satisfactory to know that it was not the cause of the loss of the case, as the nature and situation of the tumour would have rendered operation futile, however early it had been performed. All that surgical interference did was to prolong the life of a man who was dying when operated on, and for a period to restore him to consciousness.

So far as our present knowledge goes the future of brain surgery affords no hope of cure in cases like this. To have a successful issue in such operations two conditions must exist—first, an accurate localisation of disease; and, second, that the neoplasm should be of the nature of a cyst or encapsuled growth, or so surrounded, or so differentiated in structure from brain substance, that its position can be felt or determined by instruments or by touch when the eye cannot reach it. It is obviously almost impossible to discover a tumour which cannot be seen, and which, like this, is traversed by instruments with just the same degree of resistance as the surrounding brain; and, even if it be discovered, it is quite impossible for the operator to know whether it has been entirely removed.

CASE II.—E. W., tailoress, aged eighteen years, was admitted to the Whitworth Hospital, under the care of Dr. O'Carroll, on March 1st, 1890.

She was one of a family of eleven children, of whom seven were dead, and she had enjoyed good health up to four months previously to her admission, when she was attacked by pain in her right ear, followed by a purulent discharge from the meatus, and attended by impairment of hearing. She then began to lose appetite, to waste, to vomit, and to suffer from severe occipital headache. The vomiting increased in frequency, until it attacked her four or five times a day, the headache being relieved by the act of vomiting.

When admitted she was extremely emaciated, pallid, and presented an appearance indicative of great suffering and much exhaustion. There was a copious purulent discharge from the right ear, but no local tenderness or redness. She had from four to six attacks of vomiting daily, and had almost constant pain in the back of her head, which showed a paroxysmal character, and became at times so severe that it caused her to cry aloud in the most piteous fashion. She was seized five or six times a day by tonic spasm of the muscles of the back of her neck, causing extreme retraction of the head and attended by most distressing pain. These spasms were more frequent in the night than in the day, and lasted from five to ten minutes. During them her usual attitude was sitting up in bed, holding its rail, with her legs drawn up, and her left fingers alternately flexed and extended. At the end of the attack she generally turned over on her left side into the semi-prone position, her eyes at this period showing conjugate deviation to the right. She felt as if "the bed was going from under her," and was giddy when walking. Her right pupil was at times somewhat larger than her left. She had been hypermetropic from her school days. Ophthalmoscopic examination showed double optic neuritis. The reflexes were present, but diminished by emaciation. Her temperature in the axilla averaged 97°. She was partially deaf on the right side, and had right anosmia, not being able to distinguish assafœtida or iodoform with the right nostril.

She remained in the Whitworth Hospital until March 8th, when she was transferred to the Richmond Hospital for surgical treatment, her condition having become extreme, and Dr. O'Carroll having made up his mind that an intracranial abscess existed.

Examination of the case will show that sufficient grounds were

present for this opinion. The points of evidence may be summarised as follows :—

1. The previous existence of inflammation and suppuration, engaging the middle or internal ear.
2. Persistent headache.
3. Cerebral vomiting.
4. Giddiness.
5. Optic neuritis.
6. Rigidity of the muscles of the back of the neck and retraction of the head.

As to where the abscess was situated some doubt existed, as the ocalising symptoms were less definite than those of a general character. As intracranial suppuration resulting from ear disease usually occurs either in the temporal lobe, on one hand, or in or about the cerebellum, pons, or medulla, on the other, it was fair to assume that if pus were present it would probably be found in one of these situations. In favour of the pus being situated in the cerebrum, was the anosmia, which would probably indicate some engagement of the anterior extremity of the uncinate convolution of the side on which the loss of smell was observed.

On the other hand, the relation of disease to the cerebellum, or, at all events, to the parts below the tentorium, was indicated by the marked rigidity of the neck muscles, the retraction of the head, and the frequency of the vomiting—all conditions which are observed to attain their greatest development when the pons or medulla are subject to pressure from suppuration. Again, the giddiness (cerebellar incoördination) pointed to pressure on the cerebellum. Also the absence of auditory phenomena on the left side, as affording evidence of freedom of the right superior temporal convolution, gave negative inference that the temporal lobe being free, the seat of disease was probably cerebellar.

On weighing the evidence the conclusion arrived at was that pus probably existed in the temporal lobe, and that if not found there it should be sought for below the tentorium.

March 9th.—Operation, having been decided on, was performed under chloroform, and was planned so as to give access to the temporal lobe, and to permit of a trochar being passed down through the tentorium into the substance of the cerebellum, provided pus were not discovered in the cerebrum. A careful observation of Professor Cunningham's casts of the brain, and of Professor Fraser's photographs, having shown Mr. Thornley Stoker that the

point selected by Mr. Barker for his trephine opening in these cases would not expose, as he supposed, the middle temporo-sphenoidal convolution, but would expose the inferior convolution so low down as to endanger the lateral sinus, Mr. Stoker took a point higher up—that is to say, a horizontal line being taken from the lower border of the anterior bony nares through the middle point of the meatus, 1¼ inches are measured from the mid-meatus point back along this line, and from this point a perpendicular raised for 1½ inches. The base line here used, passing from the lower border of the anterior bony nares instead of from the lower edge of the orbit (Reid's), as used by Mr. Barker, gives an additional elevation of about ¼ inch, and if the point be taken 1½ inches, instead of 1¼, above it, a total elevation of about ½ inch above Mr. Barker's point is reached. A trephine opening made here will usually expose the middle convolution, and will give easy access to any part of the temporal lobe, while it is high enough to enable an operator who desires to explore the cerebellum to do so by passing a trochar downwards, inwards, and backwards, without risk of wounding the lateral sinus.

Taking the point thus indicated as a centre, a curved incision was made above it, a flap turned down, the periosteum resected from a circular area, and a trephine opening 1 inch in diameter made. The dura was then divided, when the brain-substance protruded in the fashion usually seen where undue intracranial pressure exists. The temporal lobe was next explored with a trochar and cannula 8 mm. in circumference. The instrument was introduced in various directions, as far as seemed allowable without the risk of passing beyond the lobe in front, or into the ventricular cavity internally. Puncture was made nine times unsuccessfully, but on the tenth attempt pus was found two inches from the surface of the brain, in a direction downwards, inwards, and backwards from the trephine opening, at a point corresponding to the junction of the temporal and occipital lobes immediately above the tentorium. As the pus was very thick and exuded slowly, an aspirator was attached to the cannula, and about 4 drachms of pus withdrawn. The cavity was then washed through the cannula with a solution of boracic acid, and, the cannula being withdrawn, a rubber drainage-tube 12 mm. in circumference was introduced along its track by a Lister's forceps. The dura was then replaced, the flap sutured, and the end of the drainage tube left protruding through its most depending angle. A dressing of sublimated gauze covered with a

bulky pad of sublimated wood wool was applied, and retained in position by a linen cap made to fit the skull loosely and tied under the chin by strings.

The patient got almost complete relief from pain and spasm after the operation, and had no bad symptoms until 7 o'clock next morning.

March 10th.—She became very restless, got severe occipital pain and symmetrical spasms of the muscles of the back of her neck, attended by retraction of her head. She became drowsy and semi-conscious, and experienced great difficulty in swallowing. Her temperature, which was 99·2° at the time of operation, had now risen to 100·6°. The dressings were removed; considerable bulging of the flap was found to exist, and some slight oozing of blood and brain subtance to have occurred through the tube. The tube was withdrawn, a stitch removed at each angle of the wound, and a considerable amount of clotted blood and softened brain matter pressed from under the flap. A small rubber drainage-tube was passed between the openings made by the removal of sutures, and as some heat and glazing of the skin were present, wet sublimated gauze-dressings were substituted for the dry ones previously in use.

March 11th.—Temperature 104°; not so much drowsiness; no pain in occiput, or spasm of neck; complained of pain in teeth and eyes, and of dryness of mouth and throat, and suffered so much difficulty in swallowing that she was chiefly fed by nutrient enemata and zyminised suppositories. When the dressings were removed a considerable oozing of brain substance and blood was found to have taken place.

March 11th to 22nd.—The relief from pressure now produced much amelioration in her condition. Her temperature gradually fell, and, until March 22nd, varied between 98° and 100°, reaching 101° on one day only. She remained in a fairly comfortable state until March 22nd, her chief inconveniences being due to pain in her teeth, dryness of the mouth and fauces, difficulty of swallowing, and a herpetic eruption at the right angle of her mouth. Pus began to be discharged on the 13th, and on the 17th the flap had become detached, and a hernia of the brain substance projected through the trephine hole. It was found impossible to do more than limit the size of this protrusion by moderate pressure, as when reduced and retained within the skull, severe pain and return of the old symptoms demanded that it should be allowed to protrude. On March 22nd urgent symptoms showed themselves, and

from this on to the close of the case its history is a surgical romance of hairbreadth escapes of a very remarkable character.

March 22nd.—Temp., which on the previous day had been 98°, rose suddenly late in the evening to 104°. She was seized by a sudden and violent return of the old pains in the back of her head, attended by retraction as formerly. Her condition became so extreme that it was feared she was dying, and in Mr. Stoker's absence his colleagues—Messrs. Thomson, Corley, and Dr. O'Carroll—saw her in consultation, and concluding that fresh intra-cranial suppuration was the probable cause of trouble, probed the brain in the direction of the former abscess, but without result. Some pus, however, was pressed out from between the hernia and the posterior edge of the trephine opening.

March 23rd.—This morning the girl's condition not being sensibly improved, and some stupor existing, she was put under chloroform, the hernia removed on a level with the skull by curved scissors, and on careful probing a purulent cavity found to exist between the dura and bone, posterior to the opening. This had been sealed by the overlapping hernia, and the retention of pus had caused the advent of the fresh symptoms. A rubber drainage tube was placed in this cavity, the flap readjusted, and pressure applied. The effect of the drainage was immediate relief from pain, spasm, and stupor; a fall of temperature from 104° to 98°, and of pulse from 140 to 110.

March 23rd to April 12th.—From this date until April 12th a steady improvement took place. As some tendency to protrusion of the brain still existed, an elastic bandage was applied over the gauze dressing; it reduced the hernia in a few days, and being daily reapplied, prevented any further trouble in that direction until the aggravation of symptoms acquired its removal on April 28th, after which date the hernia reappeared. On April 5th the girl was so well that she was allowed up for a short time. On April 7th the epidural suppuration had ceased, the cavity closed, and the dura united to the bone, so that a probe could no longer be passed between them.

April 9th.—Exposed surface of brain and bone is granulating in the most healthy way. Her general health and comfort is good, and no pus appears on the dressings except such as secreted by the surface granulations.

From April 13th she complained of pain in her forehead, eyes, teeth, back of neck, and right arm. These symptoms gradually

increased in severity. The temperature, previously normal, rose on the 17th, and on that and the four following days varied between 102° and 103°. On April 21st she suffered from vomiting and difficulty of breathing, and on April 21st the cause of these sufferings became evident, for on exploring the granulating brain with a probe a sequestrum from the edge of the trephine hole, forming about two-thirds of a circle, was easily found and withdrawn. Its removal, however, failed to give relief, for although the temperature fell, the pains and dyspnœa persisted, the breathing became slow and irregular, and spasms occasionally attacked the *right* arm and shoulder. She became quite intolerant of the elastic bandage round her head; it was necessary to remove it, and the hernial protrusion again took place. As localised cerebritis seemed to exist and be extending, she was on April 29th put on a mercurial course, 3 grains of grey powder and ¼ grain of opium being given three times a day. The temperature now had an average of 99°, sometimes falling to between 97° and 98°, and occasionally rising to between 102° and 103°.

May 2nd.—On removing the dressings an area as large as a sixpence of the upper and back part of the hernia was found to be gangrenous, with an aperture in the centre of the slough discharging pus. On introducing a probe it entered an abscess cavity in the brain in a direction forwards and inwards for about 1½ inches, which, allowing for the projection of the hernia, would mean more than an inch within the cranial wall. The cavity was washed out through an incision made in the gangrenous tissue, a drainage tube inserted, and gentle pressure applied. The breath having become mercurial, the grey powder was reduced to one dose per diem.

May 3rd.—The slough had localised itself, and measures 1½ in. by 1 in. The mercury was stopped.

May 4th to 6th.—Condition rapidly deteriorating; occasional cerebral vomiting; stupor passing into coma; Cheyne-Stokes' respiration; pulse 130; limbs paralysed; right pupil dilated; left contracted; conjunctival reflex gone; she appeared to be dying. At noon on May 6th a free discharge of very fœtid pus took place, followed by a rapid return to consciousness; the breathing became quiet and regular; vomiting ceased; all paralysis disappeared, and the pupils became symmetrical.

May 7th.—Discharge of pus small and healthy; general condition most favourable; hernial tendency very slight, being easily overcome by elastic pressure, which was now resumed and used without

inconvenience until the end of the treatment. The temperature became normal on May 7th, and remained without any remarkable change until May 13th.[a]

May 7th to 13th.—Between these dates very satisfactory progress was made, the abscess cavity, into which a probe could be passed for 1½ inches, was daily washed out with boric solution, and several small sloughs of brain substance thus removed. A small sequestrum from the inner table of the skull was also discharged. The amount of pus gradually diminished.

On May 13th she began to complain of frontal pain. She rolled her head from side to side, the ward seemed to her to be turning round. She could see plainly, had photophobia, diplopia, pain in eyeballs, and the gas lights appeared to be of various colours. Temperature, 103°. On the next day, May 14th, the temperature had risen to 104·6°, and the conditions just mentioned were even more pronounced.

May 15th.—A free discharge of pus took place, and proved to be the last cerebral incident in the history, for it was followed by a return to nearly normal temperature; and from this date a steady and permanent improvement took place, which has resulted in complete relief from all cerebral trouble.

The clinical history of the brain disease ends here; for, although the girl was retained in hospital until August 7th for the purpose of observation, and for the treatment of lung disease, no further head symptoms showed themselves. The wound healed completely, the flap of skin was replaced over the trephine opening by the natural cicatrisation along its edge, and the surface of the scalp over the aperture in the bone soon assumed a slightly depressed character.

In the early part of June, although no cerebral symptoms were present, a hectic temperature made its appearance, and caused much conjecture as to its cause. A cough with purulent expectoration appeared, and, on examination, a small cavity was found in the apex of the right lung. No tubercle bacillus was found in the sputum, and it was hoped that the condition might be pneumonic—a hope apparently justified; for, under treatment by dry inhalation of carbolic acid and creasote, the expectoration diminished, the temperature fell, and in August she was sent to the Convalescent Home at Stillorgan, in a very fair condition of general health.

[a] In preparing this paper for the press the history has been completed up to the present date, Nov. 10th, 1890.

She has since been kept under observation, and now, November 10th, her condition is as follows:—Her mental condition is perfect; she has no paralysis of any form; her sight is as good as before her illness; hearing in the left ear is perfect, and in the right impaired so far as is usual where the middle and the internal ear have been the seat of suppuration. There is at present evidence of lung disease at both apices. She has lately spat blood, but the tubercle bacillus has not been found in the sputum.

As regards the suppurating ear, subsequent to operation the purulent discharge was for a long time abundant. The ear was syringed twice a day with boric solution, and by the time convalescence had taken place the discharge had ceased. No local tenderness, redness, or other indication appeared which would have justified an operation to penetrate the mastoid bone.

Daily records of medical treatment in this case have not been given, because they would be tiresome, and not essential. Morphia and bromide of potassium were both used at various times for the relief of pain and spasm, and each seemed to have a good effect, and not to produce any evil consequence.

In addition to the value of this case for diagnostic and operative reasons, its clinical history has several matters of much interest, not only from a physiological point of view, but also as bearing on management, and as showing how far from hopeless even an apparently desperate case is. The engagement of the origins of the fifth and eighth nerves after the operation, as shown by the pain in the teeth, the dryness of the mouth, the facial herpes, and the difficulty of swallowing, speaks plainly as to the extent of brain substance engaged in the neighbourhood of the abscess. Again, the recovery of the brain from so extensive a destructive process as existed here is very remarkable. Not to speak of the amount of pus discharged, or of the numerous secondary abscess cavities which penetrated the brain in different directions, and each one of which seemed sufficient in itself to cause death, there was by sloughing alone, and by the removal of the hernia cerebri, an amount of direct destruction of brain substance which cannot have been less than an ounce, and may have been much more. And yet, with this extensive loss of material, no paralysis has been

produced. Clearly, the temporal lobe is largely superabundant, and capable of considerable curtailment without much ill effect.

Concerning the hernia a word may be said. The effect of the elastic pressure here was so good that it teaches how slow a surgeon should be to remove such a protrusion. It is to be remembered that when irritation and inflammation have subsided there will be abundant room in the skull for the brain substance in such a case as this, where no new growth is present. Had elastic pressure been used from the time of operation it is extremely unlikely that the outgrowth would have taken place. But whether this be so or not, it should be remembered that a hernia cerebri, although perhaps rebellious to pressure while intracranial inflammation exists, will yield readily to it when that inflammation subsides.

The case is one worthy of note for these reasons, and not less so because of the extended exploration of the brain necessary to discover the abscess in the first instance. To discover pus at a distance of two inches from the surface of the brain, and successfully evacuate it, is an encouragement to future operators.

SECTION OF OBSTETRICS.

NOTES AND REMARKS ON A CASE OF COMPLETE PROLAPSE OF THE CERVICAL ZONE OF THE UTERUS, PRECEDING LABOUR AT FULL TERM.

By FRED. WM. KIDD, M.D., &c.;
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[Read in the Section of Obstetrics, Friday, November 22, 1889.]

Patient thirty-two years of age; has been married for nine years; had three living children—the last born on the 30th of May, 1885. Has had five miscarriages. She never had any prolapse of the uterus, nor had she ever to seek for treatment for any uterine disorder; never had piles. Perinæum had been torn somewhat in first labour. Last menstruation ceased on the 17th of January of the present year (1889), when she became again pregnant. She was well and in good health until the 19th of October, on which day, about half-past ten o'clock in the morning, as she herself described it, she "got pains like first labour pains, and a lump came down in her passage." Pains lasted off and on for about an hour, when the tumour receded a little. Intermittent pains recommenced at 3 o'clock, and lasted two hours, felt low down in the back and above the pubes. Again at 7 o'clock the pains came on and the tumour descended lower than it had previously done; she thought it about the size of a turkey egg. All this time she had been about her household duties, thinking, as she had come to her full time, that labour was coming on, and that the more she moved about the better for herself.

On the morning of the 20th pains recommenced, and simultaneously the tumour descended. These pains lasted only about an hour; when they ceased the tumour receded. On the morning of the 21st slight pains occurred, but the tumour did not come

down until the evening, when it descended without any accompanying pains.

On the morning of the 22nd, when she got up at 6 30 a.m., the tumour descended, unaccompanied by pain. At 10 o'clock, however, the pains commenced—slight at first, but gradually increasing in intensity until 11 o'clock, when she said they were like "strong bearing-down pains," and were accompanied by some vomiting. She sent for the midwife, but before she arrived she states that the tumour had descended "half way to her knees," and she thought "it was the baby come into the world." All this time she was either walking about or sitting down. When the nurse arrived there had come away some sanious discharge like "the show." The nurse—herself a woman of considerable experience—thought at first that the tumour was the fœtal head with the membranes unruptured, but thought the membranes more red or raw-looking than any she had ever seen before. She put the patient to bed at once and sent for me without making any examination.

On my arrival both patient and nurse told me that the tumour had gone back very much since the patient had been put to bed. I found the following condition on examination:—A red, and in some places a semi-purple, mass protruded for about 8 inches from the vulva. The diameter of this mass was from 3½ to 4 inches; it was composed of the greatly-congested and turgid cervical zone of the uterus, dragging down with it the vaginal walls. The os was in the centre of this mass and patent, so that I could easily introduce two fingers, but nothing could be felt except a soft membranous structure, which receded before my fingers as high up as I could reach. This canal seemed somewhat funnel-shaped, with the narrow apex below; os internum was dilated. There was some slight bleeding going on, but the origin of this could not be clearly ascertained. The so-called lips of the os were quite congested, and seemed each to be 1½ inches thick, while the blood-vessels at the upper portion of the mass were very full and turgid. Taking into consideration the recent history of this procidentia, I determined to replace it if possible. Before I attempted this, the patient had several pains, which seemed to be voluntary expulsive pains provoked by the prolapse, but they were not to be compared, she said, to the violent pains she had before lying down; and during these pains, although the abdominal muscles contracted, and the uterus *seemed* to contract externally, there was no increase of tension of the membranes felt through the cervix, as would be if it were a true uterine contraction.

Grasping the cervical portion with my fingers, very much in the same manner as a paraphimosis is reduced, I used some compression, and then slowly forced the prolapsed portion upwards, until it was in the normal position. Before removing my hand, another endeavour was made to feel the presenting part, when the child's hand could just be touched. The membranes could not be found attached to the wall of the uterus as high up as the fingers could reach, although almost the whole hand was in the vagina. When the hand was removed there was no tendency for the prolapsed portion to descend again; all uneasiness and pain ceased, so the thighs were elevated by a pillow placed beneath them, and she was left quiet. By external palpation the child's head was found inclined to the left iliac fossa, and no portion had descended into the true pelvis, except, perhaps, this hand that could just be touched. Fœtal heart was heard to the right side of umbilicus.

I left her about 6 p.m., and returned at 8 p.m., and brought Dr. Fleming with me; he kindly volunteered to give me his assistance in case pains had come on and that the case had become one of true transverse presentation requiring version. There had been no further descent of the cervix, and the congestion had already begun to subside. Fearing lest labour should supervene while the cervix was still much congested, it was decided to attempt external version, so as to make the breech the presenting part, and so more gradually dilate the congested cervix with the smaller wedge—viz., the breech. This attempt was carried out with very little difficulty, except that we had to resort to combined internal and external version. Patient was left, feeling very comfortable; no bleeding whatever; no pain.

Next day, 23rd, patient felt easy; had slept well; no difficulty in micturition, but had slight sensation of bearing-down on defæcation; no rise of temperature; pulse, 88.

25th.—Had no pains since; no discharge; no return of prolapse; congestion of cervical portion entirely disappeared; os only about size of a shilling. Finding the congestion all gone, in the interests of the child I performed external version, and again brought the head down, so that it was just above the true pelvis.

On the 27th true labour pains commenced about 6 p.m., and she was delivered of a fine, strong male child at 9 30 p.m., after a very short and perfectly normal labour; at no time during the labour did any prolapse of the cervical zone occur.

Convalescence proceeded uninterruptedly, and patient was up on the 9th of November. A vulcanite ring has been put in position,

and an astringent injection used since, as there has been some slight bearing-down sensation. A probable explanation as to the cause in this case may be found in the fact that the patient was in the habit of carrying large pails of water, and had scrubbed out her room on the day before that on which she first felt the prolapse. On the other hand, she had never had any trouble from prolapse or bearing-down previously, although her perinæum had been somewhat torn in her first labour.

Before making any general remarks, I shall read the short notes of a case of prolapse of the cervix which I attended about six years ago. I was sent for to attend a soldier's wife in one of the barracks in Dublin. She had refused to go to the hospital provided for soldiers' wives when in that condition, and thereby, I believe, had forfeited her claim to be attended by the medical officer in charge. She had an army midwife with her. The surgeon had seen her the preceding evening, and said she was not in labour at all, prescribing opium. When I arrived, at eleven o'clock in the morning, I found the patient a pale, fragile, deformed creature, aged thirty-four, primipara; right knee ankylosed, and leg at right angles to thigh; hip-joints also affected by rheumatism; no deformity of the pelvis, except that it was rather small; had not been aware of any procidentia before labour came on. Pains were almost incessant, and she was forcing down as hard as she could, helping herself by pulling on a sheet tied to the top of the bed. She says she had similar pains since the preceding day, slightly relieved by the opium; liquor amnii had drained away twelve hours previously. On examination the head was found low down, distending the perinæum well with each pain, but bringing with it the cervical zone of the uterus. This was dilated to about the size of a crown, and protruded from the vulva for about one inch or more. It seemed almost cartilaginous as to hardness, and about a quarter of an inch thick, fitting closely to the head; the scalp protruding about one inch in the form of a caput succedaneum. A futile endeavour was made to push the head back a little—in the short interval between the pains. Fearing that there was danger of the uterus rupturing, so strong were the pains and so unyielding the cervix, and assisted by the

surgeon, who was surprised to find the turn things had taken—he having pronounced her not in labour—chloroform was administered. The attempt was then made to dilate the cervix with the fingers. This proving ineffectual, it was determined to incise the cervix and apply the forceps. Four incisions—two anterior and two posterior—were made with a blunt-pointed bistoury, to the extent of about an inch from the margin; short forceps applied without difficulty, and she was delivered of a living female child comparatively easily. Convalescence was quite satisfactory. There was really no hæmorrhage from the incisions, and when I examined her two months later there was no eversion of the mucous membrane of the cervical canal, nor anything to betoken what had been done, except a slight linear depression where each incision had been made. She did not suffer from any prolapse or enlargement of the cervix.

This seemed to me at the time to be a case of extremely rigid os, and that severe expulsive pains, assisted by voluntary muscular effort (from pulling on the sheet fastened to the head of the bed), had driven down the head with the rigid os closely surrounding this caput succedaneum. On reading more of the literature of the subject, however, it is just possible it *might have been* a case of hypertrophy and elongation of the cervix, *if we can believe*, first, that it could have existed to some considerable extent without her knowledge; secondly, that in two months after labour there was not a trace of hypertrophy of the cervix.

On comparing the history of these two cases it is at once evident that they are quite different and distinct from each other, both as regards causation and treatment. They may be regarded each as a type of a distinct class. There are yet two other forms of prolapse of the cervical zone of the uterus complicating pregnancy—viz., that in which the cervical portion of the womb prolapses early in pregnancy, and remains elongated, hypertrophied, and prolapsed, more or less, until abortion or labour at full term has concluded the case; and lastly, that in which impregnation has been known to take place in a uterus completely prolapsed, and go on to labour at full term.

My reason for bringing forward these cases, and troubling you

with their details, is that in all the literature on the subject that I have had access to, I have not met records of any two exactly similar cases. They are not everyday occurrences, and yet any one of us *might* meet with a similar case. In obstetrics the practitioner is more frequently confronted with unexpected complications than in any other branch of the profession. These he must be ready to deal with at a moment's notice. He should have his knowledge at the tips of his fingers, otherwise he may jeopardise the lives of both mother and child; he should know "what to do, how to do, and when to do." In most other branches the practitioner, if in doubt, can either read up authorities on the question, or call in consultation some skilled opinion, whereas in obstetrics he is often thrown completely on his own resources, by which he must either stand or fall.

Prolapse of the cervix in the pregnant uterus may take place, as I have said, early or late in pregnancy, but the predisposing causes are almost always some or all of the following—viz., weak ligaments, unusually large and wide pelvis, rupture of the perinæum in previous labour, and relaxed vaginal walls, with rectocele, cystocele, or both combined; while the determining cause is usually lifting some weight, jumping or falling on the feet from a height, or severe muscular exertion—such as washing over a tub.

In looking over the literature of this subject we meet with some very strange cases, a short account of some of which may not only be interesting but also instructive.

In the *Dublin Quarterly Journal of Medical Science*, 1853, there is a paper on "Prolapsus of the Uterus and Vagina during Pregnancy and Labour," by Mr. J. H. Houghton, of Dudley, in which he gives the following details of a case:—Mrs. S., aged twenty-six; second pregnancy. Labour commenced Nov. 13th at 4 p.m.; pains feeble until 2 p.m. following day; os then the size of half-a-crown; membranes ruptured twenty-four hours previously; head presented; os uteri close to outlet. The walls of the uterus from margin of os to where the head of the child pressed, formed a cone three inches long, apex downwards, as though the neck of the womb had dilated simultaneously with, instead of becoming

obliterated before, the dilatation of the os. The lips of the os were thick, rigid, and unyielding; pains regular and strong. Gradually head and uterus came down together, dragging with it the anterior wall of the vagina, and obliterating the anterior *cul-de-sac*. Finally the whole of the os, with a large caput succedaneum, protruded from the vagina; this went on until 8 p.m. Tartar emetic was given, and nausea kept up for three hours; then two scruples of laudanum. Os was now larger than a crown piece; pains moderated by laudanum, but returned sharply about 10 p.m. Fearing entire protrusion of head and uterus, Mr. Houghton replaced uterus as well as he could between the pains, and when the pain was coming on he passed the fingers of the right hand into the anterior *cul-de-sac* of the vagina, and the thumb of the same hand into the posterior *cul-de-sac*, fingers and thumb thus embracing the os uteri and child's head, allowing the latter to press upon and dilate the os, at the same time preventing the descent of the uterus *en masse*. At 11 30 p.m., during a strong pain, something suddenly gave way, and the child was born alive. Cervix was lacerated; uterus frequently descended during the protracted puerperium. Patient told him she had been neglected in her first confinement; had prolapsus ever since. Uterus had come down every day during her pregnancy, even to the day of her delivery. This case, on the whole, reads like one of elongated cervix, which could only have been returned into the vagina when "it had dilated sufficiently to allow of the descending head to obliterate its canal."

Dr. Merriman, in the *Medical Times*, gives six cases he has collected:—

1st. From the practice of Professor Rizzoli. Account so meagre as to render it almost useless.

2nd, in 1776, from Memoirs of the Medical Society of London.—The protruded mass was wedged in the passage, when mortification appeared imminent. The perinæum was found dilatable; it was dilated, and the mass slipped back. Child subsequently born naturally.

3rd, in 1763, in the practice of Mr. Antrobus, of Liverpool.—"She went to her full time, though the cervix became scirrhus

from exposure. First pains drew up uterus, but liquor amnii escaping it was again propelled down before the head. Counter-pressure was made, and the part lubricated; child was born and the uterus replaced."

4th, Mauriceau—"The uterus had protruded during labour, to the extent of half a foot, and was as big again as the head of a child. The os looked like a phimosis, of which the lips were three fingers' breadth, thick, and quite livid. He introduced his hand by degrees into the uterus, and thus guided the head to its exit." This seems to correspond somewhat in description to my first case, but the treatment was different.

5th, Portal.—Primipara; protrusion when os was only half a line apart. First introduced a sound, then a finger; gradually both hands were insinuated. The bag of waters assisted, and two pains delivered her; uterus was replaced. She had relaxation previous to marriage, which she could easily replace.

6th, Smithe.—The os was pushed beyond the os externum by the pains:—"During every pain I kept up child's head and the mouth of the womb, which I gradually dilated with my finger, till being fully opened it slipped up all round the head, and this by degrees gradually opening the os externum, delivery was effected."

7th, Gristock.—Third child; had suffered from procidentia since birth of last child. The protrusion became greater in third pregnancy, but as it advanced it gradually passed up. When pains came on, a large fleshy mass passed through vulva. He found os uteri and a considerable part of vagina protruded; had her fomented, and waited; when pains became strong the hand was expanded over the protruded part so as to make the ends of the five fingers afford five points of support to the prolapsed vagina; at last, during a strong pain, the mass slipped into the pelvic cavity and a child was speedily born.

Dr. David Davies, in his "Principles and Practice of Obstetric Medicine," mentions the following:—

1st. Woman, aged thirty-two, fourth pregnancy, attacked with pains like labour pains; three days elapsed and no delivery took place. Suddenly, after a prodigious expulsive effort the whole

gravid uterus and its contents protruded through the vulva; the uterus was in a state of inflammation; os scarcely patulous enough to admit the introduction of a finger. Woman died four hours after; skin was peeling off child.

2nd. Middle-aged woman, ten weeks pregnant; the uterus was pushed entirely out of vagina; it was returned and kept up by pessary until labour came on, when she was delivered of a small child.

3rd. From Haller's "Chirurgical Disputations."—"Woman, aged thirty, had procidentia for two months; woman was delivered first, uterus then replaced and retained by pessary." It does not state in this Report how far advanced in pregnancy the woman was.

4th. Uterus prolapsed outside vulva; irreducible, was fomented; pains supervened; membranes entire; hand was introduced; version accomplished, and mother and child saved. Uterus was reduced afterwards, and woman recovered perfectly.

5th. Woman had prolapsus during gestation, and experienced a repetition of it during labour owing to bearing-down too severely; protruded portion was about the size of a man's head; delivery effected by section of the os and part of the neck of the uterus; uterus subsequently reduced and retained in position by a pessary.

6th. Reported by Hanerwolf.—"The orifice of the uterus being partially dilated, the patient herself dilated it still more, the midwife assisting; birth was effected in this state of the womb; uterus, after being fomented, was returned and retained without a pessary."

Ashwell mentions two cases. One, an out-patient at Guy's, who told him that during the whole time of her last pregnancy the womb was external, and the child was born whilst the womb was outside the vulva. The woman afterwards suffered from irreducible prolapse.

The second case is from Capuron's "Maladies des Femmes," and is one of the most extraordinary on record. A peasant girl, aged fourteen, made a violent effort during menstruation, which protruded the uterus externally; it was not reduced at all. She

married at twenty-two; she enjoyed good health until she was forty, but was childless. However, one day her husband "dilated the uterine orifice, introduced the glans penis therein, and conception was the result." When labour came on the pains could not expel fœtus, owing to the cartilaginous hardness of the neck, mouth, and sides of the womb; a double incision was made, and labour terminated favourably. A full-grown dead infant was born.

Barnes, in the chapter on Prolapsus of Gravid Uterus in his "Lectures on Obstetric Operations," mentions a case, cited by Moreau, from Chopart—viz., "A young woman had procidentia uteri, the result of violence before marriage. This was never reduced, but after twenty years—the cervix becoming gradually opened—conception took place. Labour, at term, went on for twenty-four hours without progress, when the child was dead and the woman seemed expiring. The surgeon, Marrigue, divided the cervix by incision, and thus was able to extract the child; the mother recovered. It is not clearly stated that the entire uterus and child were external to the vulva at the time of labour.

There is a suspicious similarity between this case and the foregoing one, in many essential points, so as almost to make one think that they are merely slightly different versions of the same case.

There is a record of a case reported in the *Half-yearly Abstract of Medical Science*, 1869, by Mr. William Allison, which it seems very hard to credit. It is as follows:—"On 23rd March, in attending on a woman, I found the uterus containing a child, in the bed, just as if the child had been born. After sponging off any trifles of lint, &c., from the uterus, the entire mass was carefully returned into the abdomen. On 24th March a pessary was introduced, but was intolerable; so woman was desired to lie or sit still until labour came on, which it did on May 2nd; child was born alive."

In the *British Medical Journal* of July 7th, 1888, Mr. H. T. Barton, of Burnley, has reported a case which unfortunately ended fatally. Woman, aged thirty-eight, third pregnancy; had miscarried at four and five months previously; had prolapse from

girlhood. When labour came on cervix uteri completely blocked vulvar opening, and projected externally for two inches as a mass, three inches in diameter, tense and œdematous; head was felt three inches from external os. Nothing seems to have been done for her for two days, except to give 1 grain of opium and foment; then 20 grain doses of chloral were given every ten minutes, and after half an hour a seven months' fœtus was born. Patient died the next day from collapse and exhaustion.

This case, I think, teaches us that active measures are necessary if we hope to bring them to a successful termination. Might not an attempt to return the prolapse on the first day of labour, with or without incising the os, have saved this patient's life?

In the *British Medical Journal* of March 11th, 1882, Mr. Percy Boulton, Physician to the out-patients at Queen Charlotte's Hospital, reports a case very similar to that which I have previously quoted from Capuron. When the woman was last pregnant the uterus came down, between the third and fourth month, and could not be returned. The fœtus was carried to full term in the prolapsed uterus, "which reached down to the hamstrings, and was not unlike a cow's udder." Dr. Lowe, of Lynn, attended this patient, and saw both fœtus and placenta pass straight from the uterus, without, of course, traversing the pelvis. Labour was short, easy, and quick. Dr. Boulton suggests the possibility of producing artificial prolapse in certain cases as an alternative for craniotomy, and the opportunity afforded of making accurate and interesting observations with regard to uterine contraction during labour.

In the *British Medical Journal* of April 29th, 1882, Dr. J. Hickinbotham has reported the following case:—November 12th—Called to see a primipara expecting labour; found a large mass protruding from the vulva, consisting of greatly hypertrophied cervix (she also had the rare complication of a urethral cyst in front of the prolapsed cervix); os uteri admitted finger. 21st—Labour began. Although she had pains, there was no progress made; head lying at brim; os looked annular and gristly. Near the vulva cervix was encircled by large veins. Trying to dilate cervix there was much hæmorrhage, so cephalotripsy was per-

formed. Prolapse had occurred so gradually as to push aside hymen without rupturing it, and without inconvenience, so that not until marriage did she know that anything was wrong. The cause of the procidentia, he says, was, "no doubt, lifting and carrying heavy weights while still young." Six weeks after confinement uterus prolapsed, but both cervix and os were normal in appearance.

At the Aberdeen and Banff Branch of the British Medical Association the following case was reported by Dr. Shearer, of Ballater:—Labour had scarcely begun when the case was first seen. A fleshy mass the size of a large turnip, consisting of the neck and part of the body of the gravid uterus, was found protruding beyond the vulva. Entire vagina was forced outside; os, the size of a sixpence, was beginning to dilate; delivery natural. She was a multipara. The uterus had come down after the birth of the first child, and had been getting worse after each child since; it remained beyond the vulva, even when unimpregnated. It is an interesting query how pregnancy took place in this case when the womb protruded so far as to cause difficulty in walking.

Barnes, at the Obstetrical Society of London, December 6th, 1876, exhibited the uterus of a woman who had died after premature labour. The os externum had protruded beyond the vulva, with complete eversion of the vagina. Patient died of pyæmia, possibly connected with injuries sustained during labour by abnormal state of the canal the child had to pass through.

In the Obstetrical Transactions of 1874, Dr. George Roper has recorded a typical case of hypertrophic elongation of the cervix uteri at full term of pregnancy. E. C., primipara, aged twenty-two; cervix uteri protruded three inches; canal admitted forefinger; when cervix was reduced within vagina fœtal head could be felt. Os externum was freely incised in seven places; it cut with a gristly sensation; forceps were applied; living child delivered. Two months later the hypertrophied portion of cervix hung down in the vagina like a shrivelled piece of skin; it was removed lest it should again become developed by any succeeding pregnancy. In another case he was called in when the patient was in a serious

state of exhaustion. He had to deliver at once by cephalotripsy, as symptoms were so urgent, and there was not sufficient dilatation for the forceps. Patient died ten or twelve days later from pyæmic pleuro-pneumonia, caused doubtless by the local bruising and long-continued pressure.

Kleinschmidt, in the *American Journal of Obstetrics*, 1885, reports a case where the patient had had rupture of the perinæum, and the uterus came down when about seven weeks pregnant for the second time; it projected an inch, but receded in the recumbent position. After the middle of the sixth month it did not recede. Some sanious discharge occurred at intervals all through pregnancy. At full term a protrusion, which was all cervix, filled the vulva, extending beyond for two or three inches. Pains like labour pains succeeded, but there was no true contraction of the uterus. Patient assumed the knee-chest position, when the uterus, with a perceptible jerk, was replaced by its own weight in the abdominal cavity. On resuming recumbent position descent again occurred. Gave an opiate, and ordered quiet. Four days later true labour pains came on; os internum was beyond vulva, while the uterine segment enclosing the head distended the perinæum; os externum was open 1½ inches; finally, head was pushed up, and the canal opened until the head was within ½ inch of the os externum; but the os would not dilate any more, nor would the ½ inch canal shorten. Short forceps were applied. The neck of the uterus now grasped the instruments 2 inches above the handle, ¾ inch from the angle of the blades. Assistant tried to keep back uterus, which descended with the traction, instead of dilating. The noise of tearing fibre making him desist, he incised the os anteriorly and posteriorly as far as the union between the uterus and vagina. Labour was then successfully completed, and the incised cervix was sutured.

In November, 1877, Professor Simpson read a case before the Obstetrical Society of Edinburgh for Dr. Hamilton, of Hawick. Patient, twenty-four years of age, had had an abscess in the labium recently; was four months pregnant; lifting a bucket of water, complete prolapse of the uterus took place; on the fifth day labour

pains set in; the uterus could not be returned, and delivery was effected by pulling down a foot.

The lesson to be learned from the history of so many cases is, that we must understand there is no hard and fast rule to be laid down as regards treatment. The treatment must often be varied according to the exigencies of the case. In broad terms, however, the treatment should be—in every case endeavour to return the prolapsed portion. If that be not feasible, dilate the canal by hydrostatic dilators, manual dilatation, or by incision, and deliver by forceps or cephalotripsy, supporting the perinæum and vulva well, lest lower segment be drawn through. In complete procidentia, if you cannot return it, labour must take place outside the pelvis. If the uterus does not empty itself you may have to resort to the forceps or turning. You can in this case support the prolapsed portion by a sheet with a hole cut out of it large enough to let child pass through. With regard to the class where the cervix is hypertrophied and elongated, Galabin, in a paper on Prolapsed Uterus, says:—"In cases of hypertrophied cervix during an expulsive effort the swollen and hypertrophied cervix is forced suddenly through the vulva, and while it is held strangulated in that position for a greater or less time, the elastic supports of the uterus, tending to lift it again to its normal position, have a direct influence in stretching the supra-vaginal portion." Some obstetricians have advocated the amputation of this elongated cervix when it is likely to interfere with parturition; but there is one great danger which is clearly portrayed by Barnes in the chapter on Prolapsus of Gravid Uterus in his "Lectures on Obstetric Operations"—namely, that of opening into the retro-uterine peritoneal pouch, which is dragged low down by the inversion of the vagina; also in front the peritoneum descends behind the bladder to the same level as the lower margin of the symphysis pubis.

Lusk and Kleinwächter state that it is not possible for the pregnancy to be completed when it occurs in a case of complete prolapse or procidentia, because of the injuries to which the organ is exposed. Nevertheless, Stoltz states that very numerous facts prove that the pregnancy *may* arrive at term.

In conclusion, I may express the hope that some of the cases I have cited may furnish some questions of interest to my hearers, and perhaps elicit some personal experiences from members of the Section of Obstetrics in the Royal Academy of Medicine in Ireland, who have had a far more extended field of observation than I have had.

ATRESIA OF THE VAGINA; ITS PATHOLOGY AND TREATMENT.

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[Read in the Section of Obstetrics, December 20, 1889.]

The subject of vaginal malformation or occlusion, as a cause of sterility and marital troubles as well as of menstrual disorders, is one the practical importance of which is evident. Nevertheless, if in current medical literature be rightly reflected the prevailing tendencies of modern gynæcological opinion, it would seem that, whilst so much attention is devoted to utero-ovarian and tubal affections, the study of the abnormalities of that passage on the integrity of which the due performance of woman's sexual functions so essentially depends, is now somewhat unduly neglected. The following observations are therefore submitted in the hope of inducing more adequate consideration of some of those abnormal conditions of the vagina which are of unquestionable interest from the special difficulties connected with their successful treatment.

In the first volume of the Transactions of the Academy of Medicine may be found a short communication of mine on the subject of "Cicatricial Occlusion of the Vagina," and in a later volume I described certain other morbid conditions by which, whilst the structural integrity of this passage remains intact, its functional uses in relation to marital life and impregnation were impaired or destroyed. On the present occasion I purpose to consider that fortunately rarer, but graver, congenital malformation that is occasionally presented in cases of entire or partial absence of the vagina, by which the patient is not merely incapacitated for marital life, but is also subjected to great physical suffering and danger.

This subject was frequently referred to by the older writers:—"One of the most remarkable instances of Nature's wanderings from her accustomed laws," observed Dr. Davis,[a] "is that which consists in the entire absence of the vagina. Cases of this defective formation are not numerous, but they are sufficiently numerous to establish the fact of their existence. Most frequently they are found combined with absence of the uterus, as also with that of one or more of its appendages. When in those cases, says Baudelocque, there has been any passage at all, it but rarely exceeded an inch or an inch and a half in length, and has usually terminated in a *cul-de-sac*.[b] Of this variety of defective development was the case of a porter's wife, related by Morgani, "in which the external parts of generation were very diminutive, and there was scarcely to be seen a trace of hymen. The entry into the vagina did not equal the dimensions of the middle finger in any direction; the breadth of the vagina when opened longitudinally and displayed, was scarcely more than two fingers, and there were no rugæ on it. The parietes of the uterus were extremely thin, and the entire organ appeared not to have acquired any increment of bulk since the birth of its subject. There were no ovaries, a deficiency to which may reasonably be ascribed the imperfect development of the vagina and uterus."[c]

Dr. Blundell, who in many instances, though not in this, was far in advance of the gynæcological knowledge of his day, shared in the ancient doctrine as to the usual absence of uterus and general inadmissibility of any reparative treatment in these cases, and says—"When the closure above (of the vagina) is not partial, but reaches throughout the whole extent of the genital passage, the case scarcely admits of a remedy, nor indeed will the catamenia form."[d] Many years later the same view was supported by a surgeon to whom modern gynæcology owes much of its development—viz., the late Dr. Marion Sims, who says—"I have seen five cases of

[a] Davis—"Obstetric Medicine." Vol. II., p. 24. London; 1824.

[b] Baudelocque's "Midwifery." Translated by Heath, Vol. I., 215.

[c] Morgani—"De Sedibus et Causis Morborum," Epist. 46, Art. xxi. Venice, 1760.

[d] Marion Sims' "Uterine Surgery," p. 349. London, 1866.

congenital absence of the vagina, and in all of them there was no uterus."[a] Dr. Macnaughton Jones observes—"If the vagina is congenitally absent there is often no uterus as well."[b] Dr. Bousquet,[c] of Marseilles, not long since recorded a case of this kind in the Gynæcological Transactions, and in the recent American Cyclopædia of Gynæcology we find the old opinion on this point still sustained—"The whole canal may be absent, a condition which is commonly combined with absence of the uterus, but in other cases a normal uterus is found conjoined with the closed vagina."[d]

The doctrine supported by the authorities just cited would point to the general inadmissibility of reparative treatment in such cases. Fortunately, however, this often-reiterated view is, as I believe, not correct, as a general rule. Since the celebrated case reported years ago by Amussat[e] of successful formation of artificial vagina, a good many others of the same kind have been from time to time reported in disproof of former views concerning the necessary connection between utero-ovarian and vaginal developmental lesions and the impossibility of reparative treatment in the latter. Even yet, however, the number of such cases successfully treated is by no means large, and hence it may be of interest to here record three additional cases illustrating the treatment of congenital absence of the vagina. The most recent of these occurred a few weeks ago in my clinique in the Mater Misericordiæ Hospital, where the patient was admitted under the care of the senior surgeon, Mr. Hayes, by whose kindness she was subsequently, on his examination and diagnosis of the true nature of the case, transferred to my wards.

The subjoined notes are supplied by my clinical resident, Mr T. G. Dillon, to whose unremitting attention the fortunate issue of the case is largely due.

[a] Marion Sims' "Uterine Surgery," p. 349. London, 1866.

[b] Macnaughton Jones—"Diseases of Women," 3rd Ed., p. 418. Lond. 1888.

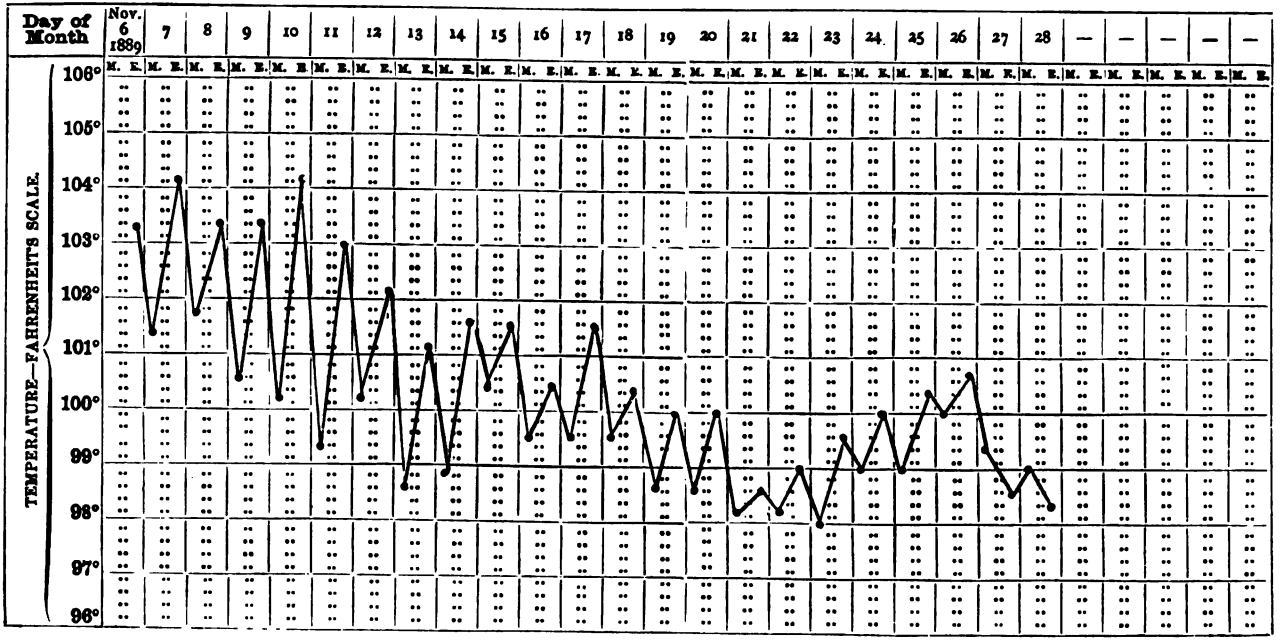
[c] "Transactions Brit. Gynæcolog. Society." Vol. I., p. 218. Lond. 1886.

[d] "American System of Gynæcology and Obstetrics." Vol. I., p. 258. New York, 1887.

[e] Amussat—"Gazette Medicale," Dec., 1835, p. 785.

Chart of Temperature,

Name, Mary G.; *Age*, 17; *Disease*, Atresia Vagina; *Result*, Recovery.



Case I.—M. G., aged seventeen, a tenant-farmer's daughter, living near Ballyhaunis, County Roscommon, admitted to this hospital 16th August, 1889.

She had never menstruated, and complained of constant pain in her back for the past three years; and on palpation a swelling as large as the fœtal head at full term before her admission had been discovered in the region of the uterus. This felt hard and movable, and was diagnosed to be a uterine tumour. At a certain period each month this swelling grew larger, and the pain in her back became more severe for a few days, when both gradually subsided. The mammæ and external genitals were well developed.

On the 3rd November a minute recto-abdominal examination of the case, under chloroform, was made by Dr. More Madden, to whose ward the case had been transferred, assisted by Dr. Duke, with the result that he discovered there was complete atresia vagina, from ¼ inch below the opening of the urethra. He decided on operating for the defect, which he did November 7th. The patient being placed in lithotomy position, a transverse incision was made through the vulval *cul-de-sac*, and thence continued upwards and backwards in what should have been the axis of the vagina by breaking through with the handle of a scalpel the dense cellular interspace between the bladder and rectum, the integrity of which were carefully guarded by the use of the sound in the former and the retention of the operator's index in the latter. In this way a passage nearly six inches in length was cautiously made to the uterus. This being globular, and no trace of the cervix or os tincæ being apparent, an incision was then made with a bistoury into most prominent accessible part of uterus, through which a large catheter was passed into the cavity, giving exit to a quantity (about 40 ozs.) of dark-coloured tarry blood, which, on examination, proved to be menstrual fluid; at the same time the tumour in abdomen began to grow smaller, and on pressing over the region of the uterus the discharge came away more quickly, which showed the tumour to be nothing else than a hæmatometra, or collection of retained menstrual fluid, which had been pent up in the uterine cavity. The uterus was then carefully washed out with an antiseptic solution, and a drainage tube introduced into the uterus, and the new vagina plugged first with iodoform gauze, and subsequently with cotton wool soaked in glycerine of borax, the plug being removed and a fresh one put in twice daily, the uterus and vagina being douched at same time through the drainage tube, until

the walls of new vagina had healed over and the temperature subsided to normal.

On the day after operation her temperature rose to 104°, her pulse was 140, and, though there was no evideuce of metro-peritonitis at any time, for some time her life hung in the balance, the symptoms pointing to septicæmia. These, however, gradually yielded to treatment, and it would be needless to follow the history of the case further, its progress being sufficiently indicated by the accompanying chart. After the first week the main difficulty consisted in overcoming by repeated dilatation the great tendency to contraction of the new vagina. Ten days after operation a quantity of blood, similar in appearance to the catamenial discharge, was passed through the drainage tube. Patient is now apparently quite well, and has a vaginal passage which easily admits two fingers. On the 7th December normal menstruation occurred.

For the particulars of the following instance of congenital deficiency of the vagina I am indebted to my colleague, Mr. Coppinger, Surgeon to the Mater Misericordiæ Hospital, by whom it was successfully operated on:—

Case II.—H. K., a thin, delicate-looking, and poorly-developed girl of eighteen, was admitted, says Dr. Coppinger, under my care into the surgical wards of the Mater Hospital, May 10th, 1889, complaining of pain in the back, and of a tumour which was supposed to represent a psoas abscess. A superficial examination and inquiry into the history of the case sufficed, however, to prove that the tumour, which was about the size of an infant's head, and occupied the hypogastric and left lumbar regions was not an abscess, but a collection of retained menstrual secretion An examination was now made under ether, when the fact that there was a complete absence of vaginal orifice, and apparently of vagina, was elicited.

On May 26th, the patient was placed in lithotomy position and with a finger introduced into the rectum and a metal male catheter, held by my house-surgeon, Dr. W. A. Morris, in the bladder, an incision was made in the middle line from about half an inch below the urethral orifice downwards for about an inch towards the anus. The recto-vesical septum was quite thin, and great care was necessary in order to avoid wounding the mucous membrane of the rectum on the one hand, or that of the bladder on the other. The two membranes were ultimately separated, for about three inches, before some loose areolar tissue was reached, and this was tunneled with the finger

until the abdominal tumour on its pelvic aspect could be felt. On pressing the latter from the front of the abdominal wall downwards, fluctuation could be appreciated. Nothing resembling an os uteri could, however, be discovered, so a trocar was thrust upwards in the middle line into the centre of the tumour. No fluid at first appeared, but, on passing a large aspirating canula along the same channel and pumping vigorously, some semi-fluid substance, jet black, and resembling tar in consistency and appearance, was evacuated.

The canula was now removed, the opening widely dilated by means of Dr. More Madden's cervical dilator, and a quantity of weak, corrosive sublimate fluid passed into the cavity by means of an irrigator. In this way about a quart of tarry fluid was gradually evacuated, a drainage tube was introduced through the new vaginal tube and new os into the cavity of the uterus, and the patient, after an operation which occupied more than an hour, sent back to her ward.

There was no subsequent rise of temperature, but great difficulty was experienced in keeping open the new vaginal tube. With this object the patient was repeatedly etherised, drainage tubes of various descriptions being used. Indeed, but for the care and attention of my late house-surgeon, Dr. Morris, the operation would, I believe, have failed in its ultimate object. In the end, however, normal menstruation was established, and the girl left the hospital cured. I may mention that I heard from the country to-day (Dec. 17) that she is in good health.

Dr. Duke, Gynæcologist to Stevens' Hospital, has kindly furnished me with the notes of another case, under his care, also illustrating the successful treatment of this condition:—

Case III.—M. C. was brought to me at Rotunda Hospital by her mother, who informed me she had never menstruated. As she was only seventeen years of age I adopted the usual routine treatment—hot baths, iron and aloes, &c.—but finding it of no use I made an examination and could find no trace of vagina, there was a dimple only where the orifice should be. The patient was poor, and I had no assistant at the operation but her own mother, who, being a sensible women, did as she was told. I administered ether, tied the patient up, and, having previously determined by finger in rectum and sound in the bladder that a uterus existed, I worked my way with the finger and scalpel up to cervix uteri, which was

elongated and with the characteristic pin-hole os. On making pressure over uterus menstrual blood exuded, but only a small quantity. I plugged the newly-made canal with wadding soaked in glycerine and carbolic acid, and after a few days inserted a glass plug. The patient menstruated naturally while the glass plug was being worn, so I removed it, and I heard the girl got good health, till I lost sight of her, as she then went to some part of the country with her relatives.

A case of obliteration of the vagina, treated under circumstances that are, fortunately, no longer possible, was reported by Dr. Upshur, of Norfolk, U.S., in the *Philadelphia Medical Examiner*, August, 1853. As that case has not, as far as I am aware, been referred to by any subsequent writer, it may be here briefly quoted :—

"Cases of this kind," remarks the reporter, "are fortunately very rare, and not more than six appear to have been reported during the last thirty years. The operation in this case is like one performed by M. Amussat, under similar circumstances, in 1836, except that Dr. Upshur completed his operation at once, whereas M. Amussat made three or four separate attempts at intervals of three days.

"The subject of this case was a negro girl, aged eighteen, the property *(sic)* of Dr. Southall, of Smithfield. She was admitted into the Norfolk Slave Infirmary, October 8th, 1852.

"About eighteen months previously she commenced to have symptoms characteristic of the approaching catamenia, recurring every four weeks, but without any discharge from the vagina. A round, well-defined tumour had gradually appeared above the pubes, becoming larger after every return of the menstrual symptoms, and giving the appearance, when she entered the Infirmary, of one advanced to the fourth month of pregnancy. Eight months ago she began to suffer from distension, and recently the pressure of the tumour upon the rectum and bladder has given her a great deal of pain.

"Upon examination, the finger passed into the vagina only about an inch and a half. On introducing one finger into the rectum, and a catheter into the bladder, the finger came in contact with

the catheter just behind the terminus of the vagina; nothing appeared to be between them but the coats of the rectum and bladder. The tumour could also be felt from the rectum, sinking low down into the pelvis, and giving decided fluctuation when gently struck.

"The operation was performed on the 15th of October. The bladder and rectum being first evacuated, chloroform was administered, and an assistant retained a catheter in the bladder, so that it could easily be touched with the forefinger of the left hand introduced into the rectum.

"The operation was commenced by perforating the centre of the obstruction with an ordinary trocar, which was pushed in to the depth of an inch, and the opening widened by a crucial incision with the scalpel, so as to admit the forefinger of the right hand. The speculum was now withdrawn, the finger introduced, and the obstructing tissue *torn* in every direction. The unyielding nature of the tissue rendered this part of the operation tedious and difficult. The speculum was then again introduced, and was arrested apparently about an inch short of the os tincæ. Pressure being still made upon the tumour, Dr. Upshur carefully divided layer after layer with the scalpel, until he reached the os tincæ, a fact which was immediately made known by an abundant flow of a fluid resembling tar in colour and consistency, and perfectly inodorous.

"This operation was followed by a severe attack of metro-peritonitis, from which she recovered; and on the 5th of November Dr. Upshur was able to make an examination, but there was so much contraction as barely to admit the forefinger up to the os tincæ; the constriction, however, was dilatable, and he introduced a sponge tent, which, in the course of a few hours, had to be removed by the nurse, but was again introduced on the next day, and borne with less inconvenience.

"On the 12th November catamenia returned, the quantity and colour being natural. On the 20th Dr. Upshur found that the contraction of the upper end of the vagina had increased; it would scarcely permit his forefinger to pass. The sponge tent was continued until she left the house on the 27th.

"On the 27th of April last the patient was again sent to the Infirmary. She stated that, until two months before, she had regularly had her monthly flow. Upon examination it was found to be impossible to reach the os tincæ, although there seemed to be an opening up to it. The speculum was introduced, and an attempt to find the opening made with a probe, but without success. No tumour could be felt from the rectum, and yet there was reason to believe that there was menstrual blood locked up in the cavity of the uterus.

"A second operation was contemplated, but it was deemed expedient to communicate with her master first. Meanwhile, however, the patient was seized with a profuse discharge from the vagina of dark, grumous blood, more fetid than the pus from a rectal abscess. This continued for five days, with entire relief to all the symptoms. Before any further examination was made, a message was received from Dr. Southall, that her services were required at home, and that he would not have another operation performed yet."

Forty-one years ago the subject of this communication was comprehensively discussed by M. Bernutz in the first volume of his work on "Diseases of Women,"[a] which, in an abreviated form, was subsequently translated by the late Dr. Meadows. A few of M. Bernutz views, and cases bearing thereon, may be here referred to as bearing on the question now under consideration, namely, the operative treatment of congenital malformations and obliterations of the vagina.

"These latter," observes M. Bernutz, "sometimes occupy the whole vagina, as in the case of M. Debrou;[b] sometimes, on the contrary, they only occupy a limited portion of that canal, and more frequently it is the middle than any other part. . . . .

"The following cases, with their *post-mortem* examinations, all exhibit the same deformity—viz., dilatation of the tubes, and the escape through them into the peritoneal cavity of the blood contained in the genital organs:—

"Cases of congenital obliteration of the vagina," says Sir B. Brodie, "are more common than is supposed. . . . Once a

[a] "Clinique Medical sur les Maladies des Femmes." Tom. I., part 1. Paris, 1848.

[b] Obs. de M. Debrou, Gaz. Méd., 1851, p. 32.

patient consulted me at St. George's Hospital; she was operated on, but would not remain in the hospital, and she died of peritonitis at her own house. In another case treated in the hospital, a short time after puncture of the vagina, symptoms of acute peritonitis set in and she died. At the *post-mortem* examination we found in the abdomen a great quantity of menstrual fluid—it was impossible to explain the presence of the liquid-blood in the abdomen other than by supposing that it had passed from the uterus through the tubes into the peritoneal cavity."[a]

The following case, recorded by M. Locatelli,[b] is also cited by Bernutz:—

"A girl, aged twenty-six, began when twenty to suffer symptoms of retained menstruation, which subsequently recurred every month. On examination there was found to be complete occlusion of the vagina at about its middle; an abdominal tumour the size of fœtal head was felt over the uterus; fluctuation was felt at the vaginal obstruction; this was incised and about eight ounces of black coagulated blood mixed with mucus was evacuted; pressure on the tumour favoured this evacuation. Peritonitis set in and death on the second day.

"On making a *post-mortem* examination there was found peritonitis with a sero-purulent effusion mixed with a little putrid blood in the left iliac fossa; the uterus was about the size of a fist; the Fallopian tubes were adherent to the ovaries and were distended with black blood. The left tube was the size of a turkey's-egg, and had ruptured."

It may not be superfluous to observe that the operative procedure necessary in attempting the formation of an artificial vagina in cases of congenital atresia of this passage is not such as may be safely undertaken without great caution and some special experience by any surgeon. Dr. Emmet says—"All writers agree as to the danger of allowing long retention of menstrual fluid, and are equally in accord as to the risk to life for the woman from any

[a] Sir B. Brodie, Lon. Med. Gazette. Vol. XXVII., p. 810.

[b] Locatelli, Sept. 1848, Gaz. Medica di Milano, cité dans l'Encyclographie, Belge. 9e serie, tom. I., p. 268. Bruxelles, 1848.

procedure instituted for the purpose of evacuating the contents of the distended organ."[a] In exemplification of this observation I may cite the following record from the practice of an ancient chirurgeon, whose boldness was apparently ill warranted by his particular skill in this instance. De Haen, by whom this case is narrated, was a distinguished pupil of Boerhaave, and, in the early years of the eighteenth century, was one of the most eminent practitioners and prolific writers of his day in Vienna, where he published in no less than sixteen copious volumes his *Ratio Medendi in Nosocomio Practico*, from the third tome of which the following case is even yet worth disinterring, as a lesson and a warning:—

"A lady, twenty-four years of age, after having tried, for eight years, such remedies as seemed best calculated for exciting the menstrual discharge, became affected with a large hard swelling of the abdomen. At length it was discovered that imperforation of the vagina was the sole cause of all the bad symptoms which the patient had long endured. An incision was made, which enabled the operator to introduce his finger into a large cavity, and which gave vent to a considerable quantity of blood. It was thought that an opening had been made into the vagina; but, the patient having died three days afterwards, it was seen that a mistake had been made, as the cavity, in which the finger had been introduced, was that of the bladder. The vagina was closed below by a substance, one inch in diameter and half an inch thick. The upper part of this passage, the uterus, and the Fallopian tubes, were exceedingly enlarged, and filled with a dark-brown, sanious fluid. A similar fluid was found extravasated in the abdomen, through a rupture which had taken place in the Fallopian tube. The ovaries were in the natural state."[b]

It should be here observed that the fatal event in De Haen's case might not, perhaps, be altogether ascribed to his unfortunate operation, inasmuch as a similar result has very frequently followed successful attempts to relieve hæmatometra—an operation which is occasionally succeeded by intra-peritoneal hæmorrhage. This,

[a] Emmet's Gynæcology, p. 209. New York. 1882.
[b] De Haen. Op. cit. Part VI., Vol. III., p. 32.

according to Bernutz, whose opinion is confirmed by Graily Hewitt and other more recent authorities, is due to the contraction of the uterus, set up by the evacuation of the fluid, continuing and forcing the blood contained in the Fallopian tubes into the peritoneal cavity.

The risk of operating in such cases with cutting instruments in the interspace between the bladder and rectum without wounding either, as in De Haen's case, was referred to by Bernutz "by way of warning against the employment of the knife in those cases, instead of using the finger, as in the case related by Amussat. "The difference," he says, "in the relative mortality of these two proceedings has induced me to separate cases of congenital absence of the vagina, requiring the process of separating the parts by means of the finger, from other cases of fibrous obliteration, congenital or acquired, in early life, for which the knife alone must be resorted to, although it is always under circumstances of extreme danger."

It should, moreover, be observed that in several instances in which no operation was attempted death had resulted from rupture of the distended Fallopian tubes, or of the uterus itself, and consequent extra-peritoneal extravasation, leading to fatal peritonitis or septicæmia. Thus, in the following instance:—

R. S., aged eighteen, was *in articulo mortis* when seen first by Dr. Munck, February 24th. For eighteen months she had suffered symptoms indicating the accession of menstruation, but without any results: these symptoms recurred periodically with increasing severity. After nine months a tumour appeared in the abdomen; it was very tender on pressure. On February 20th she felt something give way, and the abdominal tumour disappeared. Severe pain followed, attended by a good deal of febrile disturbance; a blister was applied and a saline aperient administered; but she died in three days.

Post-mortem examination.—On opening the abdomen, twelve or fourteen ounces of thick reddish fluid was seen; the peritoneum was highly congested, and traces of lymph were evident. The uterus was large but soft, and contained four or five ounces of

fluid like that in the abdomen. The Fallopian tubes were enormously distended, the free extremity of the right being closed, and a rupture of about three lines in extent was seen, forming a free communication between the uterus and the cavity of the peritoneum through which the fluid had escaped. The vagina was closed by a firm cartilaginous membrane.[a]

Another similar case was subsequently reported by Dr. Gosselin, who (*Gaz. des Hôpitaux*, 1867) relates a case of absence of vagina, with retention of menstrual blood; an artificial vagina was made, and a large quantity of fluid evacuated. The patient died on the fifth day. A large quantity of chocolate-coloured fluid was found in abdomen, and recent peritonitis. A tube had ruptured, giving exit to fluid into the abdomen.

Cases such as these that have now been referred to, whether original or quoted, are, I think, well deserving of consideration in illustration of two points of great practical interest. The first is the importance of an early diagnosis in such cases so as to anticipate and prevent the occurrence of hæmatometra to any extent that might lead to the rupture of the distended uterus or tubes. The second is the paramount necessity in all instances of operative interference of this kind to secure the permanent patency of a sufficient passage for the free external escape of the menstrual fluid, and thus obviate its possible intra-peritoneal extravasation through the Fallopian tubes.

In the formation of an artificial vaginal passage, it requires considerable caution, as already observed, whilst dissecting our way through the cellular tissue between the rectum and bladder to avoid injuring one or other of these parts. Nor is it by any means always easy to then either strike the cervical portion of the distended and globular uterus, or subsequently to maintain the permanency of the new canal.

Bearing in view the occasional absence or imperfect development of the uterus and its appendages in cases of congenital deficiency of the vagina, as well as the risks of its attempted artificial formation by operation, it is hardly necessary to add that this should

[a] Munck. Lond. Med. Gazette, Vol. XXVII., p. 867, and Bernutz, op. cit.

never be resorted to without previous evidence from physical examination of the existence of those organs, nor without the occurrence of symptoms of impeded or retained menstruation.

The *pros* and *cons* with regard to operative interference in these cases have been tersely put by Dr. Graily Hewitt:—"There are," he says, "two classes of cases to be dealt with: 1. Those in which the absence of the vagina is accompanied with signs of menstrual retention; and, 2. Those in which no signs of menstrual retention are present. This division is a practical one, for in the first class of cases operative measures are generally called for, while in the second this is not usually, or at all events necessarily, the case."[a]

In the former, by a properly-conducted recto-vesical examination, the introduction of the sound in the latter and of the finger in the former, we may readily, I think, ascertain the extent of the vaginal malformation or imperfection; whilst the condition of the uterus and its appendages can in like manner be exactly mapped out under chloroform by a conjoint recto-abdominal bimanual exploration; the result of which will be further elucidated by the history of the case, the occurrence of menstrual molimena, and in the development of the external sexual apparatus and mammæ. If these all point to existence of hæmatometra dependent on non-development or obliteration of the vaginal passage, then there can be no question as to the imperative necessity of operative interference. The mere existence, however, of the womb itself is not *per se*, in my mind, sufficient reason for operative intervention in any case of congenital absence of the vagina, unless, as previously observed, we have in addition evidence of the menstrual secretion, and of its consequent retention in the uterine cavity, where, by the impatency of its natural channel of exit, it is pent up and imprisoned, giving rise to the train of symptoms consequent on hæmatometra. In no other case, in my opinion, should the formation of an artificial vagina be ever attempted. On that point I differ from Dr. Edis, who says—"Where the vagina is congenitally absent, and so interferes with the process of menstruation, coition, or parturition, it will be re-

[a] Graily Hewitt, "Diseases of Women." 3rd Ed., p. 681. Lon. 1882.

quisite to resort to operative interference to make a passage and to maintain this in a state of patency." [a]

In the foregoing observations I have not attempted to consider the subject of acquired vaginal atresia or obliteration of the passage as a result of injury or disease. Having already sufficiently discussed these conditions in a former communication, I shall merely add that these obstructions are of much more common occurrence than is generally thought, and that my experience is entirely in accordance with Spielberg's,[b] that they most frequently result from previous puerperal endocolpitis. Neither have I here referred to those probably more frequent cases of partial obliteration, cicatricial occlusion, or atresia of the vagina that occasionally come before us in gynæcological practice, and of which, in the course of a quarter of a century's experience, I have met with some ten or twelve well-marked instances. In one of these, when I was consulted, this condition had led to the inception of proceedings for a decree of nullity of marriage; but, as in all other instances of the kind that have come under my observation, this cause of marital trouble proved amenable to treatment by gradual dilatation.

[a] Edis—"Diseases of Women," p. 397. Lon. 1882.

[b] Spielberg's "Text-book of Midwifery." Translated by Hurry. Vol. II., p. 138. Lond. 1888.

A CASE OF SUCCESSFUL CÆSAREAN SECTION.

By ARTHUR V. MACAN, M.B.;
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[Read in the Section of Obstetrics, January 31, 1890.]

Cases of successful Cæsarean section are in all countries comparatively rare, but in Ireland they have been up to the present almost unknown. Thus, I have been unable to find a record of any case where the operation was successfully performed on the living woman in this country. Nor do the records of the Rotunda Hospital during the 130 years of its existence contain one. The chief reason for this is undoubtedly that in Ireland cases of extreme pelvic deformity are very rare. I fear, also, that the fatal perfection to which constant practice had brought the operation of delivery by craniotomy and extraction, and the habit of mind it engendered of considering the child's life of no account, has naturally militated against the Cæsarean section. Finally, till the introduction in 1876 by Porro of the operation which is known by his name, the mortality of the Cæsarean section when performed in large cities or lying-in institutions was almost 100 per cent. Thus, for nearly one hundred years there had not been a successful case of the operation in Paris, though it had been performed many times by the most celebrated surgeons, and in Vienna, and Prague it had not been much more successful.

Hence, when Porro published his first successful case it attracted the attention of the whole profession, and from the success that followed the further trials of the operation, both in his hands and in those of other operators, it seemed as if the days of the old Cæsarean section were numbered. The explanation, too, of the success of Porro's operation was not difficult, for it evidently depended on the lessened danger of peritonitis, which usually followed

the Cæsarean section owing to infection of the peritoneum from the interior of the uterus. It seemed, however, also certain that infection of the peritoneum from the interior of the uterus would be impossible if any means could be devised to procure accurate union of the uterine wound. The chief difficulty was supposed to be caused by the constant alterations between contraction and relaxation of the uterine walls, and also the very quick reduction that takes place in the size of the organ after delivery. In order to overcome these difficulties Prof. Saenger, of Leipsic, introduced a new method of suturing the uterine walls. He first separated the peritoneum from the muscular layer for the distance of half an inch all round the wound, and then removed a wedge-shaped portion of uterine tissue, the base of which was formed by the portion from which the peritoneum had been elevated. He then united the muscular layer of the uterus by sutures which did not include the decidua, and then sewed the peritoneum carefully over the top. By this method, joined of course by the most scrupulous attention to antiseptics, such good results have been obtained that the Cæsarean section has again come into favour, and the operation of Porro is reserved for cases where some special conditions are present which contra-indicate the former operation, or at all events, make its success very doubtful. As successful cases of Cæsarean section accumulated it became more and more apparent that the success of the new method depended on the accurate apposition of the two uterine surfaces, and that the exact method by which this was obtained, or the material used for the sutures, was of little consequence.

Case.—K. S., aged twenty, single, admitted into the Rotunda Hospital on July 9th, 1889, having been sent up from Dundalk by Dr. Kearney. She was last unwell on Oct. 19th, 1888, and should therefore be confined about July 26th. The following are the measurements of the patient taken on admission: Height, 110·4 cm., or 3 feet 7½ inches; Sp. Il., 23 cm.; Cr. Il., 23·3 cm.; C. D., 6·25 cm.; C.V. estimated at 4·5 cm. The girl was well nourished, and had all her wits about her, and although she was so small, and the child was evidently not below the average size, and its head

had not in the least entered the brim, there was not the slightest tendency to pendulous abdomen. My first idea was to perform a Porro, in favour of which were the following points:—1st. The exact time for the operation can be settled, and one has not to wait till labour has set in. 2nd. The operation is itself much easier than the Cæsarean section, the success of which greatly depends on accurate sewing of the uterine wound, and as I had never seen either of them performed I was inclined to choose the less complicated one. 3rd. It would, if successful, save the woman from any risk of a similar operation having again to be performed. On looking up the most recent authorities I found, however, that they were in favour of the Cæsarean section, and against the operation of Porro, except when there was a positive indication for the removal of the uterus present. These are chiefly—

1. When the uterus is the seat of any disease, such as myoma, which, if left behind, might soon require removal.

2. When the woman is suffering from any disease, such as osteomalacia, the cure of which might be seriously interfered with or even prevented by a subsequent pregnancy.

3. When the patient has already been so long in labour that it is probable that the discharge is beginning to decompose, or when she has been examined by persons who may easily have infected her, or where she shows evident signs of infection having taken place. As none of these conditions were present I determined to perform Cæsarean section.

The first thing was to settle which of the methods of operating I would adopt. The chief points about which there is question are—first, should the uterus be opened *in situ*, and the child extracted, and the uterus then brought out through the abdominal walls, and sutured. Or, should the incision in the abdominal walls be made sufficiently large to allow of the uterus being turned out of the peritoneal cavity while still containing the child, the child being extracted outside the abdomen.

A further disputed point is whether an elastic ligature should be placed round the neck of the uterus, and tightened before the uterus is incised, or is it better to trust to the uterine contraction alone to stop the bleeding. Perhaps the most important point of all was as to what method I should adopt for suturing the wound in the uterus, on the accurate apposition of which, if we exclude the question of antiseptics, the whole success of the operation may be said to depend.

The only advantage of extracting the child before you turn out the uterus is that the abdominal incision does not require to be so long. What settled the question in this case was that I had determined to use the elastic ligature to check the bleeding from the wound in the uterus, and this was only possible when the uterus was first brought outside the abdomen. What determined me to use the elastic ligature was that by it all bleeding is checked, and so there is no occasion to be in any hurry when passing the sutures through the uterus. This I consider the more important, as on the accuracy with which this is done the success of the operation mainly depends. As this was my first case, and I had never even seen the operation performed, I thought that this would more than counterbalance the supposed tendency the prolonged use of the elastic ligature has to cause inertia of the uterus and consequent *post-partum* hæmorrhage. The exact time the operation is performed is also important, the most favourable period being towards the end of the first stage of labour, when the os is nearly fully dilated and the membranes still unruptured. The reason for waiting so long is in order that the lochia may be able to drain away through the cervix during the puerperal state. On July 29th the girl had some pains, and as she was already some days beyond her time, I thought labour was coming on, and sent out notices for the operation next morning at ten o'clock. However, the pains went away in the night, and it was not until August 4th, at 10 a.m., that labour commenced.

At 8 30 a.m., on August 5th, the cervix was taken up; the os admitted one finger and the F.H. was good, so notices were sent round to as many as possible, that the operation would be performed at 10 30 o'clock. Before the operation, the vagina was carefully disinfected by Dr. Bagot with corrosive sub. solut. 1:5,000, and all the usual antiseptic precautions for cases of laparotomy were adopted.

The Operation.—The incision in the abdominal wall was carried from the umbilicus to within two inches of the pubes, but had to be extended upwards considerably before the uterus could be turned out of the abdomen. The wound in the abdominal wall above the neck of the uterus was then closed as far as possible with American forceps, as this seemed to me a much easier and quicker method than by passing provisional ligatures. A strong elastic ligature was now put twice round the cervix of the uterus, drawn tight, and then fastened with a large compression forceps. The uterine wall

was then freely incised in the middle line between the fundus and the cervix. As the placenta was found attached to the uterine wall right underneath the incision, the latter was continued downwards towards the cervix, and the membranes soon came into view. They were at once ruptured and the child extracted without difficulty, but though the time from the application of the elastic ligature to the extraction of the child could scarcely have been two minutes, the child was in a state of blue asphyxia. The cord was at once tied and cut, and artificial respiration by Schultze's method practised, and the child, a fine boy weighing 7¼ lbs. soon cried out lustily.

After peeling off the placenta and membranes, and disinfecting the interior of the uterus, the next step was to bring the edges of the uterine wound together by sutures. The material chosen was silk, and the method that recommended by Prof. Fritsch, in No. 23 of the *Centralblatt f. Gynaekologie* for 1889. I was led to adopt this method from its simplicity, for except that the stitches are closer together, they are passed just the same as in closing the abdominal wall. They are introduced one cm. from the edge of the uterine wound, pass through the muscle and decidua, emerging through the latter about ½ or ¾ of cm. from edge of the wound, and continued through the opposite side of the wound in like manner. Sixteen stitches were thus passed and tied tightly, and the elastic ligature was then removed. The uterus contracted badly, and some oozing of blood took place, which was stopped by passing an additional suture, and applying hot sponges to the uterus, and kneading. The abdominal walls were then closed by sixteen stitches, and the usual Lister's dressing was applied. The patient now seemed very pale, but it was not till she was raised to put on the abdominal bandage that I was aware that she had lost a very considerable quantity of blood per vaginam. This, however, had then ceased to be more than what was natural, and so the patient was put to bed.

The after-history of the case need not delay us. The patient's temperature on the second and third day was just above 100°, and on the fifth and ninth days just touched 100°, but her pulse was very quick for the first four days, being 140 the morning after the operation. She showed no symptoms of sepsis, however, and I was forced to the conclusion that it was caused partly by the *post-partum* hæmorrhage, and partly perhaps by the five per cent. solution of carbolic acid with which I swabbed out the uterus.

The sutures were removed on the twelfth day, when the union was found to be perfect, and the girl got up on the twentieth day. More than five weeks after the operation she got a sudden rise of temperature up to 102° with a pulse of 120, which seemed to be connected with the return of menstruation, and soon subsided. I have lately heard about her, and she says she is not feeling quite strong, and that two of the uterine sutures have been expelled per vaginam.

THREE CASES OF RUPTURED PERINÆUM.

By RICHARD J. KINKEAD, M.D., L.R.C.S.;
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[Read in the Section of Obstetrics, March 7, 1890.]

So clear are the indications, so positive are the advantages of the operation, that, at the present day, to advocate the immediate repair of a recently lacerated perinæum would seem to be a work of supererogation, and to need an apology.

To those who hold that puerperal fever is in its origin solely septicæmic, the shutting of a facile door of entrance against the omnipresent, diabolically active, and poisonously powerful bacteria is positive proof of the absolute necessity of the operation, and that omitting the immediate suture of a lacerated perinæum is little less than culpable negligence.

To those whose faith in bacteria is weak, as well as to those who, although firm believers in their insinuating ingenuity for evil, yet think that puerperal fevers may be—nay more, not unfrequently are—produced from other than septic sources, the success which has followed insertion of sutures immediately after labour, thereby avoiding the danger and disagreeability of a secondary operation, has, or ought to have, been sufficient to make them adopt the operation as an imperative obligation.

Yet, and with all, the obligation is frequently neglected, and we find patients turning up from time to time suffering from the evil effects of unrepaired perineal lacerations.

Perhaps the neglect to repair arises principally from two causes:—

1st. The notion—which overlooks altogether the danger of septic infection from a raw surface—that if the laceration is not extensive it is needless to suture, inasmuch as union may, and probably will,

take place by means of rest, position, and cleanliness; that if it does not no great harm is done; whereas, on the other hand, if the laceration is extensive, more especially when the rectum is involved, suturing the wound is useless, for union is not likely to follow.

2nd. That laceration of the perinæum is an indication of incompetence, or neglect, likely to prejudice the patient against the practitioner; that if nothing is done, the fact of laceration having taken place will probably escape observation; whereas, by inserting sutures attention is called to the injury. That, therefore, it is wiser not to suture, more especially as practitioners still bear in mind the comfortable words of the old masters—that union usually takes place from apposition of the legs.

I cannot help thinking that laceration depends more on the quality of the perinæum itself and the size of the fœtal head than on the rapidity of delivery or method of support.

One meets cases in which the perinæum has the elasticity of Indian rubber, in which it yields handsomely to the strain, be it ever so great; others of that rigidity and obstinacy that, oak-like, they break, but will not bend; and others, again, that, on the slightest provocation, tear like pieces of brown paper.

In the first class, the practitioner gives himself the credit due to the perinæum, and flatters himself that his judiciously applied support, or scientific relaxation, has prevented laceration; in the second class, a tear may be avoided by incising the free margins; but in the last, nothing that can be done shall preserve the perinæum untorn.

Whether it be that higher education and social progress produce larger fœtal heads or render the perinæum more friable, this deponent sayeth not; but certainly I now find it more difficult to preserve the perinæum intact than I used to when I began to practise; and I think that when ordinary precautions are taken practitioners should not be considered in fault if laceration occurs. When, however, it does happen, then I am convinced that it ought to be dealt with like any other wound, and sutured at once.

As the strongest argument which can be used in favour of any

procedure is its simplicity and its success, I venture to trespass on the forbearance of the Section with the history of three cases of ruptured perinæum; for, although to the members it may appear like again slaying the dead, to others who may still be doubtful it offers proof of the benefits of the practice.

They are further interesting from the fact that each is illustrative of a degree of laceration, and one is specially so from the size of the child's head, and from the injury to it inflicted during labour, from which, however, it completely recovered.

Case I.—Mrs. X., aged forty-two, or thereabouts, was taken in labour, at term, of her first child; presentation vertex L. O. A.; infant a female; pelvis normal and roomy, except that coccyx was ankylosed and somewhat projecting. Labour proceeded with fair rapidity until the head reached the projecting coccyx. After waiting a reasonable time, and finding that, although the uterine contractions were strong, neither the head advanced nor the coccyx yielded—the patient being fully under chloroform—I applied the forceps. Notwithstanding support *secundum artem*, and great care in extracting, the perinæum tore down to margin of the sphincter. The placenta was expelled in about five minutes, after which two sutures were inserted.

The wound healed by the first intention, and the puerperal period was normal throughout.

Case II.—Mrs. T. was also, quâ child-bearing, well stricken in years. She had been twice married. For the twelve years during which her first matrimonial life had continued she had been barren, and now, eighteen months after her second marriage, she was in labour with her first child. Verily, as to its size, she had made up for lost time with a vengeance. She was under the care of a nurse, and I had been sent for, as, after many hours, there was no advance.

The patient was a well-made, healthy woman, with a normal pelvis. The pains were rapid, and, though short in duration, strong. The perinæum was moderately distended; on separating the labia, the vextex could be seen slightly protruding at the vaginal orifice, in which position, the nurse asserted, it had been for two or three hours; it neither advanced during the pain nor receded in the interval.

I put on Simpson's long forceps at once, expecting to have very little trouble with a head so apparently nearly born; but instead of the head extending the forceps came away with a run, the tips of the blades striking and nicking the perineal margin at each side of the raphe.

Abdominal palpation and rectal examination showed the bulk of the head to be above the brim, and the presence of the vertex at the vaginal orifice to be due to enormous elongation of the head.

I again applied the forceps, and found it necessary to make traction in the axis of the brim, the head remaining motionless, and the blades slipping with any attempt at extension. As the head gradually descended the perinæum was enormously distended. As the head came down the grip of the forceps lessened. It was impossible to get them any higher, so I passed two fingers into the rectum, and thus gradually drew the head on. So elongated was the vertex, so large the head, that when the ends of the forceps on the parietal bones were clear of the vulva the tips of the fingers in the rectum just touched the orbits.

Although extraction was exceedingly slow, and although the perinæum was drawn as well forward as possible, laceration could not be prevented. Starting from each nick caused by the blades of the forceps, it gradually tore backwards and outwards at each side as the head progressed, until, when the child was born, the lacerations extended as far back as the posterior margin of the anal aperture. The rectum was not injured, nor the sphincter torn, the rents having apparently travelled around its border: in the centre was a triangular piece of perinæum, and the wound, looked at from behind, was of the shape of a capital W.

After delivery of placenta, I inserted four sutures passed deep into tissues and through the central portion. Four-fifths of the extent of laceration healed by the first intention, the apex of the central portion alone suppurating: the puerperal period was normal.

The child was well developed and healthy, but very large; the head was the largest I have met with. When applied as far as they could go the tips of the forceps only reached the middle of the parietal bones.

The left temporal bone was fractured, not by the forceps, for they did not get so high, but by pressure against the promontory of the sacrum. There was an enormous bloody tumour over the occipital bone, and for months the infant was hemiplegic (right).

He gradually has recovered the power in the right side, and

now, at three and a half years of age, he looks perfectly healthy. He is well grown for his age; his intellectual powers are quite equal to those of other children; and any deficiency of power in the right side is only remarked by a decided tendency to be left-handed and left-footed.

Case III.—Was a primipara, aged twenty-two. The first stage was tedious; the second very rapid. As the head advanced quickly I urged the patient not to strain, and exhorted her to cry out—all to no purpose. As the head touched the perinæum she seemed to lose all control, and, as if compelled to strain, with one vigorous effort shot the child clear out of the vulva at least six inches from her on to the bed. There was no pause as the head was born; no pause for rotation; no gradual expulsion of the shoulders. As the head cleared the orifice of the vagina, rotating as it came, the liquor amnii were ejected, as if from the hose of a fire-engine, clean over the foot of the bed, while the shoulder was driven like a plough-share through the perinæum. The laceration extended into the anus, and for an inch up the rectum.

A suture passed from behind forward and through the sphincter, entering somewhat behind the diameter of the anal orifice, and passing in front of the torn bowel, to emerge on the other side opposite its point of entrance, brought the edges of the wound in both rectum and anus into handsome apposition. Two other sutures repaired the rest of the rent.

The puerperal period was normal throughout, and the wound healed by the first intention.

CASES IN WHICH LAPAROTOMY WAS PERFORMED—WITH REMARKS.

By LOMBE ATTHILL, M.D.,
Ex-Master of the Rotunda Hospital.

[Read in the Section of Obstetrics, April 18, 1890.]

Laparotomy is an operation now so frequently performed that the details of individual cases are, as a rule, of little interest. I have selected the three following out of several recently performed by me, because each presents points of special interest, while two illustrate the difficulties of arriving at an accurate diagnosis in not a few of the cases in which the presence of abdominal tumour seems to demand surgical interference.

Case I.—On the 22nd June last I was consulted by an unmarried lady, aged about forty. She stated that she had enjoyed excellent health till the preceding March, when she was attacked with severe pain extending all over the abdomen. This attack confined her to bed for some days, and she had never felt quite well since, the pain had not recurred, but she suffered from nausea and constipation, and had recently, on two occasions, fainted. The tongue was furred, and there was a deposit of lithates in the urine. She noticed too that her abdomen had enlarged considerably, menstruation was irregular and scanty.

On examining her I found that a large fluctuating cyst occupied the front of the abdomen from the umbilicus to the pubes, and was apparently adherent to the abdominal wall. This I assumed to have been the result of the sharp inflammatory attack from which she had suffered some three months previously. The uterus was normal and unconnected with the tumour, the solid portion of which lay behind it; there was not any free fluid in the abdomen. The diagnosis of multilocular ovarian tumour was made. I think it right to add that, before giving my opinion, I examined this lady

carefully under chloroform. An operation was advised, and cheerfully assented to by the patient.

On dividing the integuments, the cyst was found, as anticipated, to be intimately adherent to the abdominal parietes, and while endeavouring to separate it, the cyst ruptured, and a large quantity of serous fluid escaped. I now enlarged the opening, and passing my hand into the cavity came on a second cyst of considerable size. This I also ruptured; its contents, like that of the former, were serous. Proceeding with my examination, I found that what I had supposed to be the solid part of the tumour was a mass of colloid matter, undoubtedly of a malignant nature. I now discovered that there was no true cyst, but that a large quantity of serum had become encapsulated between the small intestines, which were matted together behind the malignant growth I have referred to, and the abdominal wall anteriorly; the whole inner surface of the latter, as well as the coils of the small intestines, were covered with flaky, unhealthy lymph. How the secondary cyst was formed I am unable to say. It seemed to me to be surrounded by the colloid growth. I removed as much of this as was possible, carefully cleansed the abdomen, and closed the wound. The patient made a rapid and perfect recovery, and in a few weeks felt so well that she believed herself to be perfectly cured—indeed her friends could hardly credit the unfavourable opinion I expressed as to her future, and it was not till the middle of February, that is eight months after the operation, that she began to exhibit signs of a recurrence of the disease.

Case II.—The second case was that of a lady, aged about thirty, married, but nulliparous. She consulted me because she suffered from menorrhagia, which had recently become so profuse as to alarm her. On examining her I found the uterus to be much enlarged, the sound penetrating to the depth of four inches, but the outline of the organ could not be made out through the abdominal walls, because a large cyst lay in front of it, and though I examined this lady under chloroform, I was unable to decide with certainty whether I was dealing with a fibro-cystic tumour or with an ovarian tumour complicated by existence of a uterine myoma. My opinion, however, inclined to the latter explanation of the case, and in this view Dr. Wm. Smyly, who assisted me, concurred. I decided to operate, but feeling that, if the uterus contained a large bleeding myoma, it might be advisable, whether the cyst proved

to be connected with the uterus or not, to remove both ovaries, I informed the patient and her husband that the operation would probably result in preventing the possibility of pregnancy ever occurring, adding, however, that the occurrence of pregnancy in her present state would be dangerous.

The operation was performed a few days subsequently. The cyst proved to be ovarian. The uterus, which was very large, was found to contain a large fibroid, which projected like a horn from its anterior surface. I therefore removed both the healthy and diseased ovary. This lady recovered rapidly. She has had at least two menstrual periods since the operation, both perfectly normal as to quantity and duration.

CASE III.—The third case was one of a very different character. The patient was an unmarried lady, aged twenty-nine. She had enjoyed good health till about five years previously to the date of my seeing her, when she began to suffer pain at her menstrual periods. Since then the pain had gradually become more and more intense and of longer duration, while of late, except for a few days in the middle of each month, she was an actual invalid and confined to bed, and even during this short interval she was a great sufferer. The cervix uteri had been divided at some time without any benefit.

On examining her I found the uterus to be perfectly normal. The left ovary was greatly enlarged, and the tube dilated. The condition of right ovary could not be satisfactorily made out, but it was very painful to the touch. I gave it as my opinion that both ovaries were diseased, and that unless they were removed no cure could be effected, but added that in the case of so young a woman every effort should first be made to alleviate her sufferings.

I accordingly prescribed hot baths and douches, the internal exhibition of the bromides with sedatives, leeches, &c., but though my directions were carried out with the greatest care, no benefit whatever resulted; and at the end of three months the patient decided to undergo the operation, as her sufferings were unbearable and her health rapidly failing.

I removed both ovaries without much difficulty; both were diseased, as was also the left tube.

This patient recovered very slowly, partly because of her extreme debility and from her inability to take nourishment, and partly from the development of various neurotic symptoms, but time has worked a wonderful change. She is now fat and healthy

and able to follow her usual avocations. She is quite free from pain, but latterly is troubled with flushings, &c., as are common in women at the climacteric period.

In thinking over the first of these cases, even with the light thrown on it by the operation, I am unable to lay down any rule to prevent the occurrence of an error in diagnosis similar to the one I made, but the operation was satisfactory in one respect, for the patient was for several months relieved of all her distressing symptoms, and, moreover, it shows that the opening of the abdomen in cases of malignant disease can be done with safety—indeed exploratory incisions seem to be no more hazardous in them than in others.

In the second case I had determined, no matter whether the tumour proved to be a uterine fibro-cyst or an ovarian tumour, complicated by the presence of a myoma, to remove the ovaries, because the patient was a young woman in whom a uterine tumour giving rise to dangerous hæmorrhage existed, and though, as a rule, I disapprove of operations of any kind in such cases, still there are exceptions, and this lady, in my opinion, was one of these. The result up to the present time has been eminently satisfactory. She still menstruates, but the hæmorrhages have ceased to recur.

The last case is more interesting to me than either of the others. I have always protested against the removal of the ovaries if it can be avoided, and my experience leads me unhesitatingly to declare that the cases demanding the operation are rare and comparatively few. The operation in uncomplicated cases is an easy and safe one, consequently the temptation to perform it is great; but putting aside its moral aspect, on which, however, I lay considerable weight, the results are frequently unsatisfactory, for not only do the symptoms for which it was performed, in not a few cases remain as bad as ever, but in many a new set of neurotic ones are developed, and the last state of the patient is worse than the first. But given carefully selected cases in which the ovaries and tubes are diseased, and that we are satisfied that the suffering depends on that condition, then oöphorectomy becomes a valuable operation, and one which it is incumbent on us to perform.

FIBRO-MYOMA OF THE OVARY.

By WILLIAM BAGOT, M.B., L.M.;
Senior Assistant Physician, Rotunda Lying-in Hospital.

[Read in the Section of Obstetrics, May 23, 1890.]

This rare specimen, which, through the courtesy of Dr. Smyly, Master of the Rotunda Hospital, I am enabled to bring before your notice this evening, is a fibro-myoma of the ovary. This tumour is of great interest and importance from a pathological point of view, because hitherto there seems to have existed a great difference of opinion as to the nature of that rare class of tumours—namely, the solid non-malignant ovarian tumour. Some observers affirm that they are fibromata; others, that they are chiefly fibro-myomata.

Spencer Wells writes that he has met with but six samples, which he considered to be fibromata.

Dr. Alban Doran, however, in his book on "Tumours of the Ovary," states that he examined one of these, which had been presented by Spencer Wells to the Museum of the Royal College of Surgeons, London, and that it was distinctly a leio-myoma, containing but little true connective tissue. All the solid tumours of the ovary that he, himself, had seen removed at operations were either sarcomata or carcinomata.

Olshausen describes them as consisting of connective tissue, sometimes with a few muscular fibres; but the latter is, he says, always scanty. Martin agrees with him. Winkel states that they are chiefly connective tissue growths. Waldeyer found no traces of smooth muscular tissue. Leopold's and Wyder's observations coincide with those of Waldeyer. Klebs and Lücke, however, found non-striated muscular fibres, as did also Hartmann, Peruzzi, and Terrier.

The history of my case is, as follows:—

CASE.—J. D., aged forty-four years; married twenty years; had given birth to eight children; eight years since last pregnancy. In December, 1889, she came to the Rotunda Hospital to have a pessary changed, as she had been wearing it for some time. The following conditions were found on examination:—Perineal laceration of the first degree; external os patulous; cervix fissured, and slight ectropion; uterus normal in size; fundus retroverted towards the left; left ovary normal. There was a solid tumour of the right ovary somewhat larger than a walnut.

The woman was in bad health, and complained of various nervous symptoms; but as none of these could be distinctly traced to the ovary, and as it did not seem to be injuring her in any way, it was not interfered with.

The patient had been under Dr. Macan's care since 1884, when the same diagnosis and prognosis had evidently been adopted. In March, 1890, however, the tumour was much larger, and, as it had grown so rapidly, we thought its removal advisable.

Accordingly, on March 27th, 1890, I removed it by an abdominal section. The patient made a perfect recovery, and all her symptoms disappeared.

Dr. Earl kindly made a microscopical examination of the tumour, and found the following:—"The tumour consisted mainly of unstriped muscle, arranged in bundles; adjoining bundles run at right angles to one another. Traversing this tissue are numerous tracts of fibrous connective tissue, rather dense; they present a somewhat insular appearance as seen in the sections. In each of these fibrous bands one or more small blood-vessels can be seen, and there appear to be very thick external coats to these vessels. At the border of these, where they abut on the muscular tissue, the distinction between the two structures is not abrupt; but they are somewhat intermingled."

OBSERVATIONS ON VISUAL EXAMINATION IN GYNÆCOLOGY AND A NEW SPECULUM ILLUMINATOR.

By THOMAS MORE MADDEN, M.D., F.R.C.S.Ed.;
Obstetric Physician and Lecturer on Clinical Gynæcology, Mater Misericordiæ Hospital, Dublin;
Physician, Hospital for Children.

[Read in the Section of Obstetrics, May 23, 1890.]

The importance of sufficient illumination as an essential element in the diagnosis and treatment of those morbid conditions of the vaginal portion of the uterus and of the vagina in which the speculum is resorted to is obvious. As was remarked by Dr. Bennett, more than a quarter of a century ago, "Whatever speculum be used for an examination, to render it satisfactory the entire cervix should be brought within the field of the instrument, and in a sufficiently good light to render evident the most trifling morbid change in the local state of the organ." Yet to the neglect of due attention to this consideration must, I think, be largely ascribed some of the erroneous views and practices which for many years retarded the progress of our branch of medicine, inasmuch as the instruments originally employed for visual examination for some years after Recamier's reintroduction of the vaginal speculum—whether Madam Bovin's ivory speculum or Charrière's quadrivales, of which I may here exhibit specimens which were in actual use within my own recollection—could afford no possibility of a distinct view of the cervical portion of the uterus by any illumination that could be thrown into them.

Our impressions on any subject, as we are told, are mainly dependent on the light in which it is presented to us; and the remark is quite as applicable to physical objects as to abstract questions. Dr. Latham long since observed in his treatise on

"Diseases of the Heart," in terms equally appropriate to uterine disease, "What an amazing difference there appears in the objects of nature around us, according to the point of view from which we regard them. When we stand on the right spot for taking in the whole prospect, we then see what before we could not see at all, and we then see clearly what before we only caught a glimpse of from some more commanding position. . . . Thus the point of view from which diseases of the heart are now regarded discloses so many new things, and puts so many old things in a much clearer light, that I distrust the results of my former experience, and feel the need of submitting all my practice, and the use of all my remedies, to the test of my own more recent observation." . . . "As diseases are better understood, and we possess surer signs for discerning their seat, and progress, and events, the records of past experience become obsolete, and so a necessity arises for a new course of clinical observations." The same eloquent writer, however, also points out, that we should "always bear in mind that though the knowledge of the senses is the best knowledge, the delusions of the senses are the worst delusions."

This observation may, I think, serve to explain the very opposite descriptions and delineations which, in the earlier days of gynæcology, writers of apparently equal accuracy and with equal opportunities of clinical experience have left on record with regard to the aspect and character of the most common forms of disease which are discernible through the vaginal speculum. For instance, a little more than thirty years ago numerous and, in some instances, very heated controversies were thus occasioned; from the time when Dr. Henry Bennett on the one side and Dr. Robert Lee on the other broke their spears and lost their tempers over the then moot question of the existence and importance of cervical ulcerations, a controversy in which, as in most other similar discussions, each was perhaps equally in error, and neither could see the truth as it has been since disclosed to us by improved methods of investigation, to which I venture to think that appliances such as that which forms the subject of this paper may prove subservient. Looking at the cervix by the means thus relied upon Bennett

asserted that ulceration of the os uteri was the most frequent pathological condition disclosed to view by the speculum; whilst Lee no less strenuously denied that this was ever seen in any case. Thus, in the Transactions of the Royal Medico-Chirurgical Society for November 1st, 1857, Dr. Lee went so far as to say that "neither in the living nor in the dead body had he ever seen a case of simple ulceration from chronic inflammation of the os or cervix uteri, and to apply the term to states of the os uteri in which the mucous membrane, or, as it is termed by some, the basement membrane, is not destroyed by ulceration, was an abuse of language calculated only to deceive and mislead the members of the medical profession, from whom the truth had been carefully concealed. The speculum emanates from the syphilitic wards of the hospitals at Paris, and it would have been better for the women of England had its use been confined to those institutions."

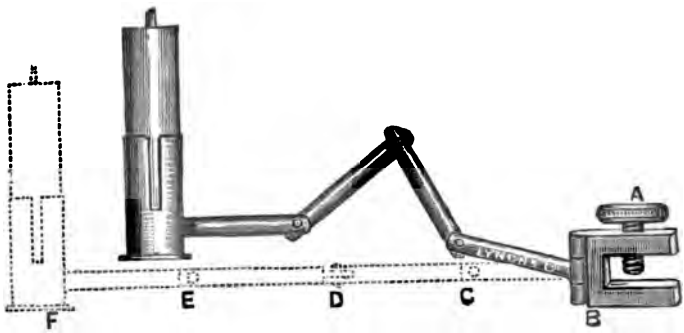
Nor was this emphatic negation of the character of the morbid conditions depicted as ulcerations of the os and cervix in Bennett's treatise on uterine disease—and many of which presented themselves as such to him and his followers until their true nature was ascertained long subsequently by Dr. Emmet, and which were previously regarded as ulcerations, mainly because they were inspected through a badly-lighted speculum—sufficient for the uncompromising Obstetric Physician of St. George's Hospital, but it led him, as just said, to the length of denying the utility of the instrument through which such erroneous views of uterine pathology had been obtained. Shortly afterwards Dr. Lee republished his views in "A Treatise on the Employment of the Speculum in the Diagnosis and Treatment of Uterine Diseases, with 300 cases. By Robert Lee, M.D., F.R.S., Obstetric Physician to St. George's Hospital, &c. London. 1857." This was, as he says, issued "with the hope that it may contribute in some degree to the correction of those erroneous and pernicious doctrines respecting the diseases of the uterus which have recently been introduced into England from foreign countries; and, if possible, to curb the licentious use of the speculum."

Had, however, those who thirty years ago thus differed as to the

nature of the morbid appearances presented to visual examination by the vaginal speculum, and entertained such widely opposite opinions as to the value of the means by which they were disclosed, possessed instruments such as the various forms of specula which, from the time of Fergusson and Marion Sims, have been developed and brought to their present perfection, and enjoyed the advantage of those methods of illumination by which direct sunlight may, when necessary, be now replaced, the violent controversies and erroneous practices of former days might possibly have been avoided.

Much as has been thus done in this way, there is, I think, still room for further improvement, as even yet amongst the every-day troubles of gynæcological work not the least frequent or least annoying of its kind is the difficulty occasionally experienced in making a satisfactory visual examination of the vagina or vaginal portion of the uterus in many cases where it is required for diagnostic or therapeutic purposes. In regions wherein sunshine is so exceptional, as unfortunately is the case under the leaden skies and murky atmosphere of the British Isles, this difficulty frequently presents itself even in the best-arranged consulting rooms, where the couch is most advantageously placed with reference to light. And, *à fortiori*, it occurs still more commonly in the patient's chamber, where the bed is often so situated as to preclude full access of natural light into the speculum. Nor is this deficiency supplied by any of those electric speculum lamps that I have myself at least as yet employed, these being apt to fail us at the moment their aid is needed; as, owing to some one or other of the defects either in battery, connections, or lamp that are of such continual occurrence in electric apparatus, on pressing the button, instead of the brilliant flood of light expected the result obtained may be either *nil* or else merely the dull-red glow of the incandescent carbon filament. Whilst on the other hand, if we content ourselves with the more reliable, if less elegant "bit of candle end," still recommended by some authorities for this purpose, the necessity for holding it so as to throw some light into the speculum must largely interfere with any manipulation required by the case.

I would, therefore, venture to suggest to other practitioners who are likely to meet with the difficulty just referred to, a trial of the little appliance now shown, which I have found serviceable under such circumstances. It consists simply, as will be seen, in a very portable, many-jointed light-holder, capable of rotation in every direction, which can be instantly and securely affixed to any form of speculum so as not to be in the surgeon's way, whilst affording sufficient and reliable light for all examinations or operations in the vaginal passage.



I have only to add that this instrument has been made for me by Messrs. Lynch & Co., of Aldersgate, London, who have very satisfactorily carried out my directions in the construction of a speculum illuminator which I hope may possibly be found as useful and handy by other surgeons as it has been by myself.

ON THE TREATMENT OF IRREDUCIBLE RETROFLEXED OR RETROVERTED UTERI BY RECTO-ABDOMINAL MANIPULATION, WITH NOTES OF A SUCCESSFUL CASE.

By ALFRED J. SMITH, M.B.;
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Examiner in Midwifery, Royal University of Ireland.

[Read in the Section of Obstetrics, May 23, 1890.]

It is not my intention to occupy the time of this Section to any length. I merely wish to bring under your notice, and invite discussion on the treatment of irreducible retroverted or retroflexed uteri by recto-abdominal manipulation, a method which has found such favour and given such good results, not only to its originator, Professor Schultze,[a] of Jena, but also at the hands of many of his followers both in America and on the Continent. An irreducible retroflexed or retroverted uterus is one, we know, which cannot be reduced to its normal position of anteflexion, but which permits of a variable degree of mobility.

Retroverted or retroflexed uteri become irreducible from the presence of various tumours, or from the formation of adhesions or cicatrices in Douglas's pouch. With tumours the essential urgent indications are given by the tumours themselves, and, therefore, their treatment does not form part of the paper. I shall confine myself chiefly to the far larger class, where cicatrices, or adhesions, are the only obstacles to a successful reposition.

Cicatricial contractions, when they involve the superficial layers of the peritoneum in the pouch of Douglas, do not, according to Schultze,[b] admit of forcible separation. This variety is a most

[a] Zeitschrift für Geburtshülfe und Gynækologie. XIV. Band, I. Heft. 1887.
[b] Dr. Macan's Edition of Schultze's Displacements of the Uterus, p. 223, s. 140.

deceptive one, which, forming as it does a persistent obstacle to the maintenance of the uterus in its normal position, offers no great impediment to its temporary reposition. The same may be said of some connective tissue contractions, and that following the absorption of the effused fluid in hæmatocele. If you meddle with them, except to employ deep massage, or, perhaps, to remove the appendages, you will have reason to regret your interference.

Peritoneal adhesions resulting from a mixed gonorrhœal infection are always associated with enlarged or dilated tubes, and consequently are at once excluded from the domain of recto-abdominal manipulation.

Uncomplicated peritoneal adhesions—*i.e.*, those which are formed by the organisation of fibrous exudation between the posterior surface of the uterus and the pouch of Douglas—alone are suitable for forcible separation. They are met with under three different varieties—as cords, or tendrils, which vary in length; as bands, which are either broad or narrow; or the fundus may be glued down, as it were, by flaky adhesions to the anterior rectal wall.

The technique of Schultze's special method of separation by recto-abdominal manipulation I will now describe by reading the notes of a case in which it was employed:—

Case.—Mrs. —— consulted me on February 25, 1889. She was nine years married; had never been pregnant; had never suffered from any severe attack of illness; had a good family history.

History.—She complained of pains in the right and left iliac regions, in the small of the back (sacral region), also suffered intensely both shortly before and during menstruation, which lasted one week, there was aggravation of the pain on the the first day of the flow; slight leucorrhœa; was most anxious to have a family. She had been for four years under the care of a specialist, who employed pessaries, glycerine, vaginal plugs, and hot-water douches, without any permanent benefit.

I was quite unable to examine patient in my study, as any attempt caused too much pain. I proposed, and she readily consented to an examination the following morning under ether, which my colleague, Dr. Bagot, kindly gave for me. I placed her in the

dorsal position, with the thighs flexed and abducted, and the bladder and rectum emptied. I found external genitals normal; vagina normal; cervix looking downwards and forwards; fundus absent from front, but easily mapped out in posterior *cul-de-sac* in a condition of retroflexion about the level of the third sacral bone; it could be elevated bimanually to the level of the first sacral bone putting on the stretch numerous fine bands, and dragging forward to a slight extent the anterior rectal wall. It returned to its old position directly my hands were removed. I could not reduce it to the normal position. To the right of the fundus, and slightly posterior, was a body about the size of a walnut, also bound down by adhesions, but allowing slight motion. This tumour proved to be the right ovary; because, on examining the right broad ligament and fornix, the ovary was found absent from its normal position, and the ovarian ligament could be traced into it. The right Fallopian tube was normal in size, but directed posteriorly. On the left side the Fallopian tube was slighty thickened, and the left ovary was displaced backwards and towards the middle line; it was perfectly movable. Having douched out the vagina with plain water I passed a uterine sound into the uterus. The internal os was patulous. Length of uterus 7·5 cm. Its interior was smooth; no blood or glairy discharge on drawing away sound.

Diagnosis.—A retroflexed and irreducible uterus bound down with fine long adhesions to the anterior rectal wall, and complicated with prolapsed and adherent ovary. Dr. Bagot agreed with my diagnosis, and that it was a most suitable case to try the separation of the adhesions by recto-abdominal manipulation.

Having irrigated the rectum, I passed the index and middle fingers of the right hand into it above the inner sphincter, and the thumb of the same hand into the vagina. With the left hand acting through the abdominal walls I grasped the fundus of the uterus and lifted it up, thus defining with remarkable clearness the adhesions which prevented its complete reposition, and mapped out the utero-sacral ligaments which passed away on either side in the form of a crescent, gradually disappearing towards the sacrum. With the fingers in the rectum, placed between these sacral ligaments and kept close to the fundus of the uterus, which was steadied by the external hand, I separated the fine adhesions, tearing through some, breaking through others, by a side to side motion, just as one separates the placenta. The force employed was slight, and was regulated by the density of the adhesions.

The separation was accompanied by a peculiar creaking sensation like new leather, produced, no doubt, by the friction over the freshly-separated surfaces.

In separating the ovary from its bed I experienced some difficulty in finding an opening, as recommended by Professor Schultze, where one by insinuating the tips of the middle finger gradually enucleates the ovary. In this manœuvre I was greatly assisted by changing my hands. Great care was taken to avoid pulling on the ovary or using unnecessary force. This part of the operation delayed me most, as I was naturally afraid, not knowing how much force to use; but my perseverance was rewarded by a complete separation of the ovary.

Having satisfied myself that both uterus and ovary were now completely free from adhesions and reducible, I introduced into the vagina a No. 8 Hodge pessary, modified to increase the posterior curvature, in order better to insure keeping the ovary and uterus in position. I then washed out vagina and rectum, and put in a morphia suppository, ½ gr.

9 p.m.—Complained of tenderness over lower part of abdomen (due to bruising), and pain around umbilicus. Temperature, 99°; pulse, 108. Ordered hot poultices; ice to suck. Gave ¼ gr. morphia hypodermically.

Feb. 27th.—Passed fair night; complained greatly of flatulence; no appetite; very thirsty. Temperature, 98°; pulse, 96. Ordered spt. am. aromat., spt. cajaput, āā ♏ 20, aquæ ad ℥i, to be taken at once. Hot weak tea to relieve thirst. Passed catheter.

P.M.—Temperature, 98·4°; pulse, 96. Pains in left side quite gone; uterus tender on pressure. Catheter passed.

Feb. 28th.—Had a good night. Ordered vagina to be syringed with hot water.

March 3rd.—Examined per vaginam. Found vagina stretched considerably by pessary, but causing no uneasiness; fundus anteverted; right ovary lying on right arm of Hodge, tender on pressure. Ordered hot vaginal douches of a temperature 105° F.

March 13th.—Patient got up, feeling fairly strong; no pains either in sacral or iliac regions; pessary changed; vagina douched out; can examine without causing pain; a smaller pessary—7½—introduced. Hot douches to be used once a day.

April 12th.—No pain with change, which is the first she remembers without pain.

July 26th.—Pessary changed; uterus anteflexed; right ovary

prolapsed, quite movable, no tenderness on examination; constipative. Ordered mist. ferri sulph.

Oct. 20th.—Spent three weeks with her husband shooting, and never felt even "back weary." Pessary changed.

Dec. 16th.—Pessary removed.

Dec. 17th.—Uterus retroposed.

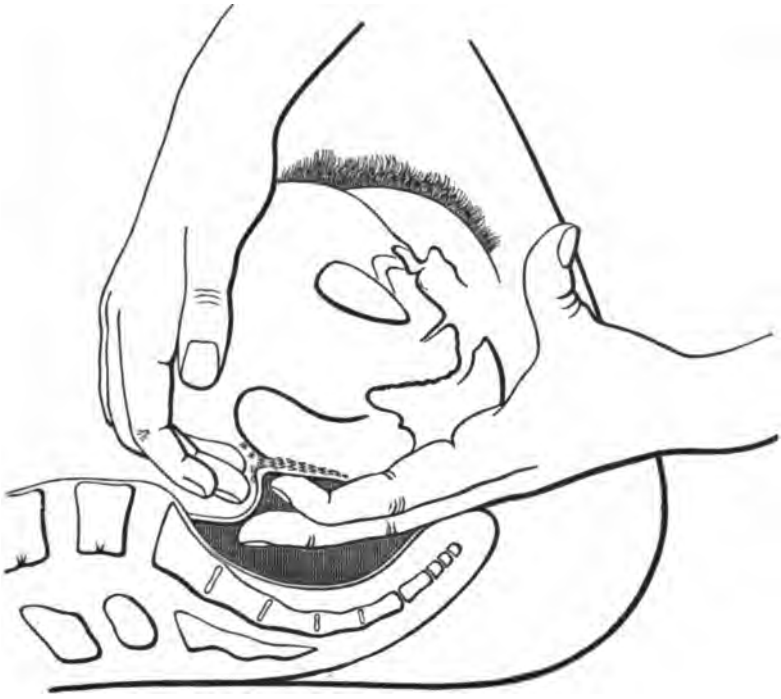
Dec. 20th.—Uterus retroflexed. Pessary put in again.

Feb. 22nd, 1890.—Uterus anteverted; ovary resting on arm of Hodge. Says her changes cause no discomfort, and that life is worth living.

April 26th.—Pessary changed; feels quite comfortable.

I have seen the patient at intervals of three months, and she continues in excellent health.

I did not use an ice-bag over the lower part of the abdomen, as recommended by Prof. Schultze, and a modified Hodge pessary was substituted for the figure of 8.



The method of separating flaky adhesions differs slightly from the method above described, and is at once made clear by referring to the diagram given by Schultze in the *Zeitschrift für Geburtshülfe und Gynäkologie*, and here introduced; it seems more difficult and, to my mind, does not commend itself in practice—abdominal section in this variety is far safer. However, Schultze claims for it marked success, particularly in recent cases.

The dangers of recto-abdominal manipulation are—1st, hæmorrhage; 2nd, peritonitis, local or general.

Hæmorrhage.—In every case there must be a slight amount of oozing from the freshly separated surfaces, but any extensive hæmorrhage from rupture of vascular adhesions is more of a theoretical possibility, and has never given any trouble—at least in the recorded cases. I remember a case in the Rotunda Hospital where an attempt to separate adhesions was followed by an hæmatoma in the right post fornix; it gave no trouble, but disappeared in the course of a few weeks without any aggravation of the symptoms.

Peritonitis, except when it is set up by the bursting of a pyosalpinx, or through rupturing old encysted streptococci nests, is never met with; trauma does not cause it unless assisted by infection. Peritonitis is an accident which seldom occurs, and will, I believe, never occur if a thorough examination and selection is made before proceeding to separation.

In presenting this special line of treatment for your discussion, I hope I shall not be understood as recommending a line of treatment of a very novel or dangerous character, or that I am introducing any innovation. I find numerous references to its successful adoption in the literature both of America and the Continent.

Erich,[a] of Baltimore, reports seven cases of retroflexion of the uterus, with peritoneal adhesion, between the fundus and the pouch of Douglas treated by forcible separation. His method differed somewhat from Prof. Schultze's—it consisted in dilating the cervix with sponge tents, and then forcibly bursting the adhesions, with the

[a] American Journal of Obst., Vol. XIII., Oct., 1880.

sound acting as a lever. Although he was not completely successful in all his cases, he nevertheless had no bad results.

Hegar and Kaltenbach[a] recommend the forcible separation, but the method must be tried by the manual method.

Fritsch[b] relates how he employed Schultze's method in two cases with splendid results.

Dr. Macan,[c] in his inaugural address as President of the British Gynæcological Society, stated that in some cases separation—bimanually—can be made easily, and though the patients may suffer pain afterwards, he never saw any bad results follow.

Mundé and Wells[d] say it should not be forgotten that in many cases of fixed retroversion or retroflexion satisfactory results may be obtained without cutting, by the employment of Schultze's separation method.

Strong[e] records 20 successful cases.

Gottschalk[f] speaks most favourably of this separation method, which he has employed successfully, though some cases required repeated sittings—a point the necessity of which he especially insists upon.

This operation is not one of universal application—to urge this view would be absurd; but what I hold is that when employed in properly-selected cases—*i.e.*, where the adhesions are fine, band-like, or cordiform, and where we have excluded tumours, distended or infected tubes, we have no operation so safe, or one that gives such good results in the treatment of irreducible retroversion or flexion as that of recto-abdominal manipulation. The patient must be thoroughly examined under an anæsthetic, and the adhesions or obstacles which prevent the successful reposition of the uterus must be accurately mapped out before any attempt is made at forcible

[a] Die Operative Gynækologie. Erlangen, 1886. 3 Aufl., S. 132.

[b] Billroth. Handbuch Frauenkrankheiten. Bd. I. I. Aufl., S. 129–130. II. Aufl., S. 760–761.

[c] The British Gynæcological Journal, May, 1889, p. 29.

[d] The Annual of the Universal Medical Sciences. Vol. 2. F. 5. 1890.

[e] Boston Medical and Surgical Journal. July 11th, 1890.

[f] Centralblatt für Gynækologie. Jan. 19th, 1889.

separation. This precaution cannot be too strongly insisted upon; there must be no leap in the dark. Unless you can define precisely the nature and extent of the adhesions, and exclude the possibility of distended or infected tubes, the case is not one in which to try separation.

SECTION OF PATHOLOGY.

TYPICAL FRACTURES.

By E. H. BENNETT, M.D., M.Ch., F.R.C.S.;
Professor of Surgery in the University of Dublin.

[Read in the Section of Pathology, November 1, 1889.]

The study of typical fractures may help to advance this branch of surgery. I think that much has yet to be learned by us all before we can make our diagnosis and treatment of fractures creditable to surgery. How discreditable to us are the vast majority of the specimens which fill our museums! If any doubt this, I would ask the sceptic to take his pick and choice from our museum shelves, and hunt up, as we can in our catalogues, and note the names of those who have treated the lesions during life. Many a pupil will blush to see the work of his master, and of this I am certain, however he may pride himself on his own better and newer methods, if he test his work by the product of the macerating tub, he will find himself in no better position than his predecessors. To improve this state of things I think we must improve our mode of study of this branch of pathology. If by any means we can make the study of fractures easier, more exact, and more interesting than it is at present, some of these opprobria of surgery with which our museums abound may be removed; or those that cannot be removed may more clearly be condoned. The help that I would offer is the direction of attention to the study of typical fractures.

In order that I may clear myself from a charge of self-conceit, or of talking mere platitudes in speaking to you about such a subject, which many of you may on the first view regard as an

integral of the A B C of surgical pathology, let me quote from one of the most recent writers a passage which sets forth very clearly the position which the study of type holds in the modern surgical literature of fractures. Packard says, in his article in Ashurst's Encyclopædia—"If anyone studies a large number of fractures, placing them in series according to the portion of the skeleton involved, he may readily note a certain uniformity which prevails among the different members of each series. Variations do indeed exist, but they are traceable to differences, perhaps slight, in the character, direction, or exact mode of application of the fracturing force, or in the conditions of resistance, or perhaps in the shape of the bones themselves. The dominant lines of breakage are singularly constant, and although they have been pointed out in regard to certain special fractures, they have been overlooked in the general study of these injuries."

In this passage the term typical fracture, as I use it, is clearly defined, and the degree of development of its study is truly marked down to the year 1884.

The character of the type is not rigid, but variable within certain narrow limits. "Facies non omnibus una nec diversa tamen qualem decet esse sororum." The essential basis of this study is a tripod—first, the shape and structure of the bone; second, the character of the fracturing force; and third, the character of the fracture.

When we have established by a sufficient pathological series the features of the type we can assert that the remaining legs of the tripod are present. The bones of the skeleton are fairly constant in form, in the distribution of their strength, and in their modes of degeneration.

They are liable to exceptional strain from external violence and from the action of the muscles attached to them. We find that under these exceptional strains which exceed the powers of their resistance "the brittle strength of bones" gives way and fractures result. The typical fracture being found, the constancy of the strength of the bone admitted, it follows that the force in any given instance must be a constant also.

Fig. 5. Fig. 4. Fig. 3. Fig. 2. Fig. 1.

Fig. 1. United Ordinary Fracture. Fig. 2. Recent Ordinary Fracture. Fig. 3. Fracture External to Trapezoid Ligament, united by Fibrous Tissue. Fig. 4. United Fracture of Sternal End. Fig. 5. Experimental Fracture of the corresponding Bone.

Put aside from our present consideration bullet injuries and great smashes of the limbs or trunk in machinery accidents and the like, and let us look to the more common fractures that daily fill our hospital wards and form the bulk of the united fractures of our museum collections. I make this limitation only to confine the scope of my address, not because the greater compound injuries of the skeleton are devoid of type. Their types are more difficult to trace, and neither the time at my disposal nor my own knowledge of them would justify my entering on so vast a search. All the world of surgical writers are agreed that fractures are caused by direct or indirect injuries, by muscular action, or by avulsion. Many have been content to carry the study of force thus far only, to state that a given fracture is the result of one or other of these kinds of force, and there leave the matter. I hold that this extent of knowledge, though in itself good, helps us but little in dealing with fractures at the bedside, which, after all, should be our great object.

Often we find that the determination of the kind of force which gives rise to most typical fracture has wholly escaped the notice of writers, or has been lost from view by the prejudice derived from absurd anatomical theory. For instance, the majority of writers attribute the ordinary fracture of the clavicle (Figs. 1 and 2) to indirect violence, which acts, according to their theory, in the axis of the bone. They say, "the part between the two curves is generally the weakest part of the bone, and hence more liable to fracture." Is there any ground for this statement as to the strength of the bone? None that I know of; it is a mere assertion. I recollect well being taught that the clavicle broke in the ordinary manner because the pressure of a shock acting in the axis of the bone increased its curves, and the break must therefore be placed where the longitudinal axis cuts these curves. This is a theory quite different from that I have just mentioned.

Here is another, from one of our standard authorities:—

"Most frequently the great convexity is broken, the bone bending here when pressed upon its extremity, the curve becoming increased, and at last giving way. This fracture may arise from

direct violence, but usually is the result of falls on the hand or shoulder."

In this a fresh site is selected, and the force may be direct or indirect. These passages show that opinions are very various, and even contradictory, with regard to the most common fracture of the skeleton. They will with greater certainty be found so when less common lesions are under examination. Before leaving this particular example we may pause a moment to see the types of fracture in the several districts of the clavicle, and, it may be, appreciate the value of the mode of study I advocate as an aid to diagnosis.

The ordinary fracture results from shocks very various in their kind, but reduced to order by the road by which they reach the clavicle.

When a man breaks his collar-bone in this fashion, it matters little whether he fall on his hand, his elbow, or the point of his shoulder. The force acts through the shoulder-joint and glenoid of the scapula, and the brunt of the battle is seated where the coraco-clavicular ligaments unite the bones. The clavicle gives way inside these always obliquely, and with only such variations as one can see must result from the great variety of strains possible.

In the typical fracture of the clavicle next in frequency (Fig. 3), that seated outside the coraco-clavicular ligaments, the bone breaks transversely; the force acts on the outer part of the spine of the scapula, either by the individual falling backwards, or by his being struck a heavy diffused blow on the same part.

Next in order of frequency is the fracture between the ligaments, caused by falls or blows, taking effect on the upper surface of the shoulder, or when the bone is enfeebled by atrophy by the mere weight of the upper limb; in this the clavicle breaks over the coracoid process as one breaks a stick on the knee.

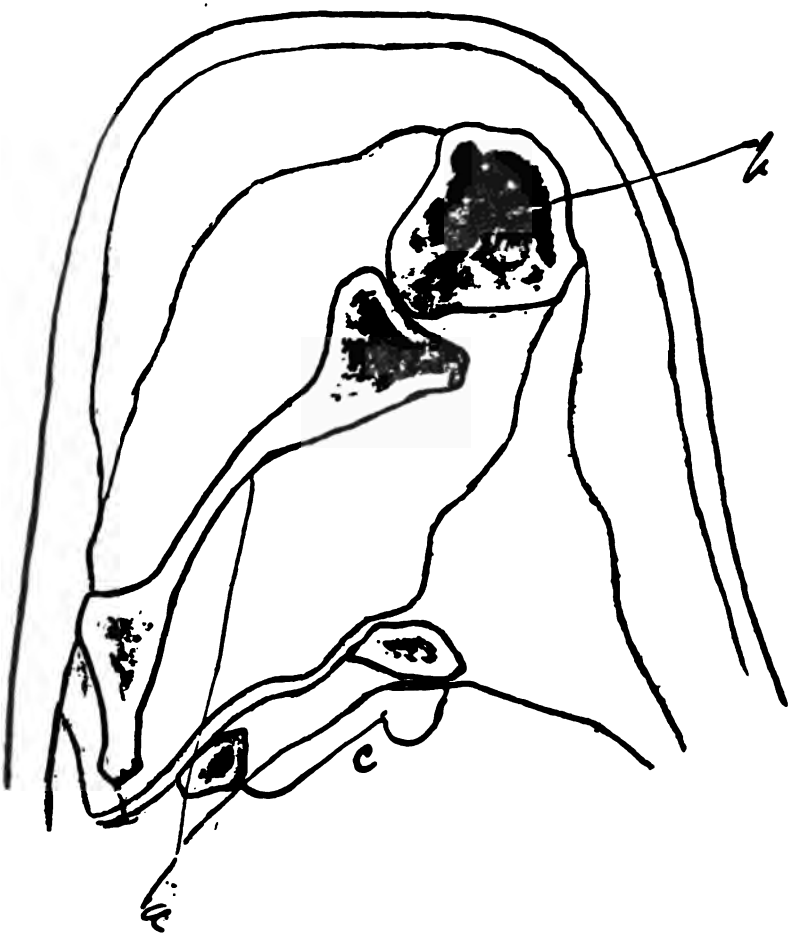
The sternal end of the clavicle breaks with constant obliquity under a purely axial pressure (Figs. 4 and 5). A direct blow will break the bone anywhere where it strikes, and as is the rule in long bones, the lesion is transverse to the axis of the bone at the point struck.

Fig. 7.

Recent Vertical Fracture of the Scapula.

For all these assertions I have given, I think, ample proof, pathological, clinical, and experimental ("Annals of Surgery," Vol. I.), nor has any one yet shaken my faith in their truth. Are not these studies of type a help at the bedside? Could we extend the like research to other bones, might we not also gain more accuracy of diagnosis in their lesions?

Fig. 8.



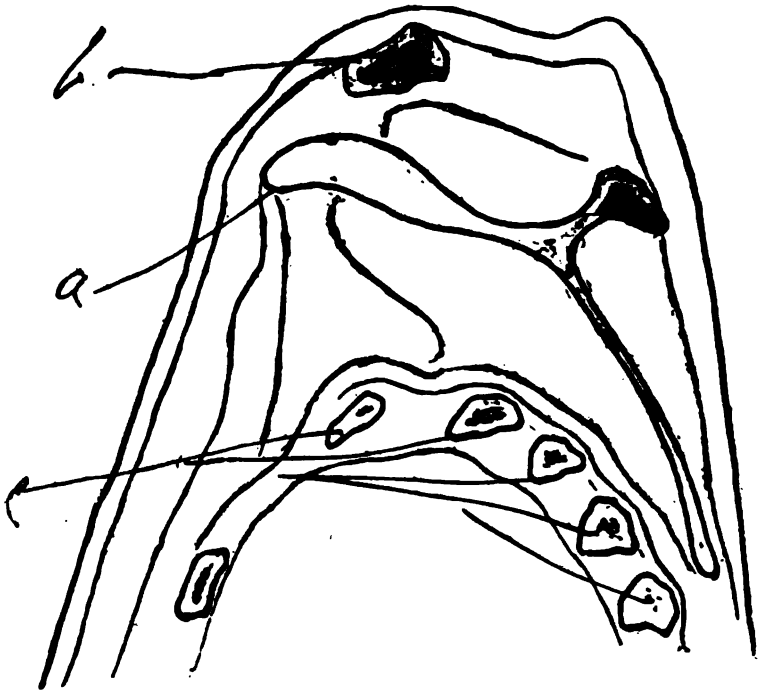
Horizontal Frozen Section of Scapular Region through the Shoulder-joint. *a*, Scapula. *b*, Humerus. *c*, Ribs.

Let me turn for a moment to another illustration.

In regard to fractures of the scapula (Figs. 6, 7), I hold that our textbooks are astray. Without exception, they assert that fractures of the body of the scapula are produced by direct injury.

Does it not seem strange, in view of the vast variety of forces that produce these lesions, that our pathological collections should prove that two great types of fracture are observable, and that all the ordinary fractures of the body of the bone conform to these two?

Fig. 9.



Frozen Section vertically carried through the Shoulder
a, Scapula. *b*, Clavicle. *c*, Ribs.

What is it that reduces, so to speak, forces acting in very varied directions to such limited results? I say the curves of the

Fig. 10. Fig. 11.

scapula. When a man is crushed beneath the mass of a falling pile of timber or of masonry, or a wheel passes over his shoulder as he lies prone on the ground, his scapula, supported on its edges by the trunk and outside by the shoulder, bears the pressure, and yields transversely (Fig. 6) or longitudinally (Fig. 7), according as either arch feels the strain. These, then, are all fractures from indirect force, and they are reduced to type by the constant form and shape of the bone. One clinical fact attests this. Did anyone who has treated these fractures ever see them compound, as they must frequently be if they always result, as our books say, from direct injury?

Turn to the leg for another illustration. A direct blow on the shin will break the bones of the leg transversely; an indirect strain by fall on the feet or otherwise breaks the bones obliquely; the oblique fracture will obviously be the more common. I heard once a great teacher say, perhaps with a savour of hyperbole in his assertion, to accentuate this relative frequency, "Of one hundred fractures of the leg, ninety-nine will be oblique from above and behind, downwards and forwards." This overstates the case, but it expresses the typical character of these fractures familiar to every surgeon (Fig. 10). I saw the other day an evident transverse fracture of both bones of the leg. Asking the cause, the patient asserted that he had slipped on the kerb-stone and fell. He was ready with every detail of his fall. His statement was much at variance with my views, which I have put before you. I discussed them with my class. Next day the solution of the difficulty appeared; the leg was broken by a kick inflicted by a policeman, from whom our patient sought to rescue a prisoner. Fearing arrest and detention in Mountjoy Prison instead of Sir P. Dun's Hospital, the ready lie, attested by many oaths, was offered to shake my faith in typical fractures.

In this situation, again, we see how great importance attaches to the exhaustive study of type. M. Gosselin has shown that when the oblique fracture varies its plane so that the letter V becomes fairly developed on the subcutaneous surface of the tibia, although the fracture is seated far from the ankle, and no overt

sign discloses a fracturing of the lower fragment, there passes a fissure to the ankle along the back of the tibia, opening up its cavity while it lacerates the medullary cavity of the lower fragment throughout its entire extent. Hence the high risk to life which attends this injury, and also to the ankle-joint. When I read M. Gosselin's paper, a good many years ago, I started off to test the question in our own museum, in the collection of this college, and in the Richmond Museum.

The examples were not many—some four (Figs. 12, 13), I think; but wherever the apex of the V lay fair in the centre of the subcutaneous surface of the bone a fissure also passed spirally to the ankle, and of the four one at least had been submitted to amputation.

M. Gosselin has not touched the question of the cause of this special lesion. I have seen it in the living as the result of a twist of the limb caused by the patient turning suddenly while his foot remained fixed against the side of a ladder. A similar cause is assigned for the injury in the case recorded by M. Chassaignac.

Again, exceptional characters in a fracture may catch our attention and lead to the extension of our knowledge. Everyone will admit that oblique fractures of long bones, as a rule, unite with marked deformity; in none more so than in the ordinary fracture of the leg.

Years ago I came across some very marked exceptions to this rule in the fibula, and not understanding them, I put them by for further examination. As time passed I got enough to satisfy me that a family likeness pervaded all. I then saw that to answer the question I had set myself I must get the two bones of the leg in such cases. A little patience and a few years gave me enough evidence to establish the fact that in certain strains of the ankle the fibula breaks habitually in its upper part, and being held secure, unites free of deformity. I have verified the injury and its mode of occurrence, and now examples accumulate, and each recurring year submits one or more examples of this lesion to our hospital class. Already sixteen pathological specimens expose in our museum this clearly typical fracture (Fig. 11).

Fig. 12.

Fig. 13.

Fig. 14.

Syphilitic Clavicles, yielding exceptional Fractures.

I fear I have tired my audience with this dry subject and these weary words, but I hope that I have at least shown that the study of the subject of fractures, based on a strict examination of the form and strength of each bone, the character of the fracturing force, with its direction, and lastly, the form of the fracture, may help us in clinical diagnosis, in prognosis, in treatment, and in medico-legal investigation.

I have touched but little on the variations of the first of my data. I could, did time permit, show that fractures of bones vary their type under the influence of structural changes induced by rickets, by syphilis (Fig. 14), and by atrophy, and the growing bone in health has its own special forms of fracture.

But enough; the geometrician can, from sufficient independent data essential to the triangle, construct the figure. I have attempted to show that, given any two of the limbs of my tripod, the surgeon may in face of the vast majority of the fractures of the skeleton deduce the third, and bring this knowledge to his aid under very varied emergencies of study or of practice.

CASE OF FRACTURE OF THE SKULL, WITH ESCAPE OF BRAIN-SUBSTANCE; HERNIA CEREBRI.

By THOMAS EVELYN LITTLE, M.D., L.R.C.S.;
University Anatomist, and Surgeon to Sir Patrick Dun's Hospital.

[Read in the Section of Pathology, November 1, 1889.]

The case I wish to detail (the specimens of which I exhibit) is one of fracture of the skull, in a child three years of age. The accident occurred on August 11, 1889.

The mode in which the accident occurred was this:—The father and mother of the child had been quarrelling—as their manner was—and, as an incident in the fight, the difference of opinion having been renewed after the mother went to bed with the child sleeping beside her, the husband seized a wooden stool, and flung it violently at his wife. His aim not being very steady, for reasons which the hour of the night—it was one o'clock—rendered easily intelligible, and the room being badly lighted, the missile missed the mother, but struck the child on the right frontal region. The infant, when it was recognised to be injured, was immediately carried by another man, who was present, to Sir Patrick Dun's Hospital.

The condition of the patient on being brought to hospital was as follows:—A considerable wound was observed on the right frontal region, which was bleeding slightly, and from it some lacerated brain-substance had escaped, and was escaping—to such an extent, indeed, that even the sleeve of the coat of the man who had carried the infant, and upon whose arm the poor child's head had rested, was bespattered with portions of the brain. Notwithstanding this, the child presented no symptoms of collapse or of concussion of the brain. It was quite conscious—so much so, indeed, that it resisted,

and fought with the house surgeon on his attempting to examine the wound.

When I first saw the patient—about eight hours after admission—on removing the dressings from the head, it presented a scalp wound in the situation just below the right frontal eminence of about three inches in length, which was slightly gaping, and was stripped upwards from the bone, the periosteum of which was separated. The wound was full of broken-up brain substance and blood, which had also insinuated itself underneath the scalp-flap. On gently probing the wound with the finger, the sharp edge of a fracture of the calvaria, which extended backwards under the scalp wound, could be detected, with some, but very insignificant, depression. The size we had pretty accurately mapped out as about that of a florin. As to whether this portion of bone was fixed, or completely separated, I could not then satisfy myself, by any amount of investigation which I deemed to be justifiable; but it became subsequently obvious that it was fixed at the posterior part. The patient presented a total absence of any cerebral symptoms whatsoever. The child was quite observant and intelligent, and had not had, nor then had, any of the symptoms of cerebral concussion or cerebral irritation. There was no paralysis of any kind, or any muscular spasm; and sensation everywhere—as far as capable of being tested in so young a subject—was unimpaired.

Progress of the Case.—I do not intend to go through, in detail, the particulars, as recorded from day to day in the hospital case book, but shall just mention the main features which became developed in the case.

The patient lived to the evening of the 15th day, dying then after the supervention of a convulsive attack—due (as we had diagnosticated it to be) to the occurrence of a diffuse arachnitis.

The phenomena which had developed in the interim, and on each of which I shall say a word or two, were:—Further and continued escape of brain-substance; discharge of serous fluid from the wound; hernia cerebri—gradually growing; divergence of head to right (2nd week); condition of wound; and mode of death.

The escape of brain-matter continued for some days, appearing

on the successively-removed dressings up to the fifth day, after which it was no more observed.

The discharge of serous fluid from the wound—fluid which, from its character and mode of occurrence, was obviously analogous to, and had the same source as that we are so familiar with as escaping from the ear in cases of fracture of the skull's base—was a feature in this case; and was remarkable in one particular—*i.e.*, its intermittence; it was tolerably copious primarily, for about the first forty-eight hours after the injury, when it ceased. It then recurred, to a slight extent and for a short time, on the seventh day, when it again subsided. For the last two days of the child's life, however, it again appeared with extreme copiousness, deluging and soaking the pillows and bedclothes.

A hernia cerebri—the occurrence of which we had anticipated, from the small size and obliquity of the wound in the dura mater—commenced to make its appearance on the eighth day, and gradually grew in size up to the time of death, when it had assumed about the size of half a large walnut. On the morning of the evening of death a large part of this fungus separated while dressing the wound; and, having been preserved in spirit, Dr. R. G. Patteson, who kindly undertook the investigation of the specimen, informs me as the result of his examination:—"It proved to be merely a piece of broken-down brain substance and blood-clot mixed; no trace of membranes."

The symptom of a persistent spasmodic divergence of the head to the right side (that of the injury) which appeared, and was present during the last week of the child's life, is one which I think more especially worthy of note, as being the only solitary motor phenomenon developed during the progress of this case, up to the occurrence of the final and fatal convulsion. There was no associated deviation of the eyes, which were easily and voluntarily movable in all directions. Upon this symptom I shall make a very brief remark further on.

As to the condition of the wound, it appeared to assume readily a very healthy character, which it maintained throughout. There was no deep-seated suppuration, or bagging of pus—in fact no

suppuration except at the very edges, which were healthily granulating, and cicatrising at the time of death. There was at no period any offensive odour; and, indeed, as far as the physical senses can be a test, the wound remained aseptic.

The mode of death was by the supervention of a severe and protracted convulsive attack, which, although apparently commencing by a twitching of the limbs of the left side, was afterwards much more marked on, and eventually confined to, the right side.

Post-mortem Examination.—(The calvaria, brain, and membranes were exhibited). On examining the body after death, we observed a condition of things, which is in chief part to be still seen in the specimens before the meeting.

The position, size, and condition of the *scalp wound* were as I have described them above.

The *calvaria*, with the dura mater attached, are here to be seen. Just at and below the right frontal eminence there is a fracture of the skull, of approximately circular shape, and of about the size of a half-crown piece, slightly depressed in front. Anteriorly and laterally, the bone is completely and sharply broken, but behind there is no fracture, or solution of continuity of bony tissue, the bone being here simply gradually bent inwards. A somewhat sharp point of the bone anteriorly, it can be seen, has penetrated and wounded the dura mater, a rent in which exists, of about half an inch in length, somewhat obliquely situated with regard to the opening in the bone. The depression of the bone was only to the extent of three sixteenths of an inch.

On examining the *brain*, we observe a deep excavation in the frontal lobe, corresponding in position and size to the skull fracture. It extends for the depth of fully an inch into the substance of the brain, is remarkably circumscribed, and is unassociated with any extent of laceration, or softening of the brain tissue in the neighbourhood. The surfaces of both cerebral hemispheres are covered with a layer of greenish-yellow lymph, beneath the arachnoid, following the course of the sulci over the sides, and even to a slight extent the base, of the brain. It is, curiously

enough, somewhat more extensive and diffused over the left than over the right side.

Remarks.—This case suggests some very interesting and important points, with reference to the subject of the particular form of fracture of the skull of which it is an example.

In the first place, the extraordinary immunity from primary (and, indeed, often of even any subsequent) symptoms of a serious character after severe and extensive brain injury, enjoyed by children of a tender age, is markedly exemplified. I calculated that this child must have lost, from first to last, at least half an ounce of cerebral tissue; and yet up to the final and fatal convulsion, with the single exception I have mentioned—of the persistent deviation of the head to the side—there was not, during the whole course of the case, a single symptom of interference with brain function. Of course, too, the situation of the cerebral destruction—*i.e.*, almost wholly situated in the frontal region, in front of the now recognised motor area of the brain cortex—must be taken into account. We all know that the fact of the occurrence of extensive brain lesion in the frontal region without serious symptoms is a very old observation—one, *i.e.*, long antecedent to any of the modern definite ideas as to cerebral cortical localisation. But, in this individual case of mine, the situation of the lesion, taken in connection with the same exceptional and single motor phenomenon to which I have alluded (deviation of the head to the right) is in no small degree interesting and deserving of a few moments' attention to details.

It will be remembered that the recent observations of Ferrier, Horsley, and others, have located a small area of the cortex of the frontal lobe, occupying the posterior extremities of the superior and middle frontal convolutions, just in front of the ascending frontal convolution, as the focal centre of what is termed the "conjugate deviation of the head and eyes," to the same side. Now, if we compare the exact position of the lesion in my case with this localisation, we observe that it (the latter) is just at its anterior part encroached upon by the posterior edge of the site of the fracture. The explanation, however, of the reason why,

in this case, we had *not* (as I have mentioned, and as we certainly had not) the associated divergence of the eyes, I must leave to those who are more familiar with the interpretation of the minutiæ of cerebral localisation than I pretend to be. It may be that this case may suggest that the anterior part of this region is concerned with the lateral movements of the head, and its posterior part with the associated eye movements.

With regard to the laceration and escape of brain substance, which occurred to such an extent in this case, there is, I think, one small point of some slight importance—I mean with regard to precise mechanism of the brain extravasation. We know that laceration of the brain may take place from injury in different ways—thus, *e g.*, it may occur as the direct result of the blow fracturing the skull, driving in, and perhaps comminuting the bone; or it may occur from cerebral *contre-coup*, at the opposite pole of the brain cavity. In the present case there is, however, at first sight, an apparent difficulty in explaining the amount of brain destruction, and of brain actually lost. As you see, there is no competent depression, and no comminution, or driving inwards of the bone. It is—when we particularly examine the fracture, and the size of the opening into the brain cavity—scarcely conceivable that the amount of cerebral substance which is lost can have escaped *primarily* through such a minute, and such an oblique, opening in the skull and dura mater from any simple internal pressure. I think, then, that the probable interpretation lies in an appeal to the nature of the injury, the age of the patient, and the precise details of the pathological lesion. The subject is a young child, where the bones of the calvaria are notoriously thin and pliable; and looking again at the specimen before us, you will observe—as, indeed, I have already pointed out—that the fragment of broken bone is bent in, and (as it were) hinged inwards (without being fractured) at its posterior part. Considering, then, the nature of the injury, and the force with which the blow must have been delivered, associated with the altogether disproportionate depth to which we observe the wound of the brain to have extended (especially when compared with the very slight amount of

bone which remains depressed), is it not a probable—indeed I should say almost only—solution of the difficulty that this hinging inwards of the bone (as I have ventured to call it) may have taken place to a much greater extent at the moment of the blow, and that the elastic infantile bone immediately springing outwards may have lacerated, and (so to speak) dug out, or spooned out, the large amount of brain matter found in, and in the neighbourhood of, the wound; and which we can still see to be missing in the specimen? The amount of brain matter actually removed, with the very slight depression which existed, readily explains, also, another point—viz., the absence of symptoms, or rather the non-existence, of any brain pressure.

There are some interesting questions in reference to the treatment in this case, which might be discussed; but these I do not look upon as available, or suitable for discussion in this Section of the Academy.

CYSTIC DERMOID OF CONJUNCTIVA, WITH COLOBOMA PALPEBRÆ.

By JOHN B. STORY, M.B.;
Surgeon to St. Mark's Ophthalmic Hospital.

[Read in the Section of Pathology, November 1, 1889.]

Bridget B., a baby, six weeks old, was sent to me in May, 1888, by my friend Dr. Ringwood, of Kells. On admission to hospital the child's mother gave the following history:—At birth a small whitish spot was noticed on the white of the left eye, near the inner corner, quite close to the inner angle of the lids. This has been growing ever since, and growing towards the "sight" of the eye. It reached the coloured part of the eye very shortly, growing more over it every day. At first healthy white was to be seen between the coloured part of the eye and the little growth.

On admission to hospital a small tumour was present on the nasal side of the left eye. It was of a whitish colour and solid-looking aspect. It consisted of two parts—a spherical or hemispherical portion touching and adherent to the periphery of the cornea, and a smaller spherical body superimposed upon the former. From the apex of the latter there grew a small hair. A small, half-moon shaped piece was, as it were, scooped out of the margin of the upper lid, corresponding to the position of the tumour, and a smaller but distinctly marked loss of substance existed in the lower lid in the same situation. The cilia in both lids were also displaced, so that their line was broken, and they lined the edges of the little excavations. The tumour was excised, the anterior solid-looking portion being first dissected off the cornea; then a strabismus hook was passed easily backwards under the conjunctival or scleral portion of the tumour. This hook passed underneath a long cystic-looking band of tissue, with some dark spots scattered

through it, which extended from the tumour proper as far back as the insertion of the internal rectus muscle. It adhered to the muscle itself, probably to the belly of the muscle, before the tendon passed through the capsule of Tenon. On breaking through the adhesions the new growth came away.

Dr. J. Alfred Scott very kindly made a microscopic examination of the parts removed. His report is as follows:—"The small tumour was somewhat pear-shaped, about 12 mm. long and 6 mm. wide at its widest part. It was composed principally of connective tissue containing many small blood-vessels, and some striped muscle at the smaller end of the tumour. It was covered by a prolongation of the conjunctiva, the structure of which was nearly normal. In one of the sections a small hair, with well-formed sebaceous glands, was noticed. A small cyst existed also about 4 mm. × 3 mm., which was lined by a single layer of flat epithelium."

The position and microscopic characters of this growth justify us in assigning it to the class of dermoids, and the peculiarities of this particular example consist in the occurrence of a cyst within the growth, and its connection with the belly of the internal rectus muscle. These growths generally occur on the corneo-scleral margin, and if the history is to be relied on, the present case is exceptional in starting in a portion of conjunctiva some distance removed from the corneal margin. The association of colobomata of the lids with conjunctival dermoids is a well-known fact, and there can be no doubt that the two congenital defects have a close etiological connection. The view of De Wecker appears the most probable, that there is an arrest in the normal histological metamorphoses which transform, or should transform, the embryonic skin into conjunctiva. As a consequence, a dermoid growth appears upon the eyeball, and an interference takes place in the due development of the corresponding eyelid, so that a coloboma palpebræ also results.

A RENAL CALCULUS WITH AN UNUSUAL NUCLEUS.

By KENDAL FRANKS, M.D., Dublin, F.R.C.S.;
Surgeon to the Adelaide Hospital.

[Read in the Section of Pathology, November 29, 1889.]

Foreign bodies not infrequently form the nuclei of vesical calculus, but it is, I believe, an extremely rare thing—if, indeed, this specimen is not a unique one—to find a foreign body forming the core of a renal stone.

The specimen is composed chiefly of phosphate of lime, with a small amount of carbonate of lime, and in this respect agrees with nearly all the renal calculi which I have myself removed, or which I have seen removed by operation. The smaller fragment (see Fig. 2) was accidentally broken off by the forceps during the pro-

Fig. 1.

Fig. 2.

cess of extraction, and it was the separation of this piece which revealed the point of the nucleus, jutting out from the end of the larger fragment, and which is clearly the point of an ordinary sewing needle (Fig. 1).

The patient from whom this specimen was obtained was a young man, aged twenty-two, who first consulted me on October 14th,

1889, for a sinus in the right loin, just above the crest of the ilium and about four inches behind the anterior superior spine of the ilium. This sinus was daily discharging large quantities of pus, sometimes amounting to nearly a pint. He told me that in January last (1889) he went to South Africa. Whilst there he developed a train of symptoms which led the doctors in Durban to diagnose a stone in the right kidney, and for which they proposed an operation. With this object he entered the hospital in Durban. He informed me that several punctures were made previously to operation, on several occasions, in order to ascertain the presence of a stone, but all with negative results. An operation was undertaken and an incision made parallel to the crest of the ilium, about an inch above it, and two inches in length, but no stone was reached, and it is doubtful if the kidney were reached at all or explored. A drainage tube was inserted. He remained in hospital for three months, and about the end of August returned to his native air near Dublin. The sinus never closed in the loin.

When I first saw him he was a fine-looking, well built young man, presenting, nevertheless, the cachectic appearance of one suffering from prolonged suppuration. There were two small openings in the loin—in the scar which I have mentioned. An unusually long probe passed readily in to its full length without meeting with obstruction. In addition to this he had an inflamed swelling in the right groin, which at first I thought was an ordinary bubo, but on his lying down and having slight pressure made with the finger, its tenseness disappeared, and it became evident that it communicated with the suppurating cavity in the loin. The urine was quite healthy, and presented no deposit on standing. I advised him to undergo another operation, and to this he at once consented. It was performed on October 18th.

When he was under the influence of the anæsthetic and placed in the proper position, I made the usual incision in such cases, beginning a little below the twelfth rib, and just external to the erector spinæ muscle, and terminating a little above and behind the anterior superior spine. It was five inches in length, and was well above the original incision, neither the extent nor the position

of which commended itself as being calculated to attain the object in view. I soon reached a great deal of dense cicatricial tissue, and, dissecting through this, came upon the deeper parts of the sinus, which I followed up. I soon came upon a large cavity filled with pus, situated immediately behind the right kidney, and in this cavity I found the stone.

The posterior surface of the kidney could be felt, and as it appeared quite healthy, it was not further interfered with. The abscess cavity was found to have burrowed downwards and forwards, and a probe passed down into this pouch could be felt in the swelling in the groin. This was undoubtedly the result of the erect position he had maintained for a long time. A drainage tube was inserted in the loin and the wound well irrigated and dressed in the usual way. The patient made an excellent recovery; the wound quickly cicatrised, and the old sinus, which had been thoroughly scraped and cleaned, has been closed for nearly three weeks. I found it necessary, however, about four weeks after the operation, to make a small opening in the groin, from which a very small quantity of pus escaped.

Now, the interesting question in this case is, How did the needle get into the kidney and thus induce the formation of a stone? When I first discovered the needle, it was a few days after the operation. At first it occurred to me that it might have been left there during the operation in May; but this hypothesis is untenable on two grounds: first, because the point of the needle shows that it is apparently an ordinary sewing needle and not one of those usually employed by surgeons; secondly, because the operation in May was undertaken on account of the diagnosis of renal calculus having been made; and, although no stone was then found—owing, I believe, to the position and limited nature of the incision then made—still, as the later operation has shown, the stone actually did exist, and in its centre, forming the nucleus, is the needle in question. Dismissing, therefore, this hypothesis, I carefully questioned the patient, but he could not remember having in any way introduced a needle into his body. However, he related the circumstance to his sister, and she at once reminded him that

in 1872, when he was a little boy, he had been sent home from school and was kept home for some days because he had swallowed a needle. As, however, it failed to produce any symptoms or to give rise to any sort of annoyance, no more was thought of it, and he was sent back to his studies.

Now, the probable solution of the problem seems to me to be this: That in the well-known erratic peregrinations of needles introduced into the body, the one in question, after wandering about for a time, found a resting place in the cortical portion of the right kidney. It there became encrusted by the deposit of phosphates from the urine and so formed a stone, and this very encrustation served to protect the needle from itself being destroyed by a slow process of oxidation. Evidently it gave rise subsequently to considerable irritation; an abscess formed around it in the renal substance, and this bursting into the cellular tissue behind the kidney, gave rise to the perinephric abscess which we discovered at the operation, and in which this calculus lay.

The stone weighed 3·01 grammes, and Dr. Piel, assistant to the Professor of Chemistry, R.C.S.I., informs me that it is composed of:—

Phosphate of calcium - - -	69·83
Organic matter and water - - -	16·65
Carbonate of lime, etc. - - -	13·52
	100·00

UNITED FRACTURE OF THE BASE OF THE SKULL.

By E. H. BENNETT, M.D., M.Ch., F.R.C.S.;
Professor of Surgery in the University of Dublin, &c.

[Read in the Section of Pathology, November 29, 1889.]

Last May I submitted to this Section of the Royal Academy an ununited fracture of the patella, which I had obtained during the *post-mortem* examination of a man who died in epileptic convulsions at Sir Patrick Dun's Hospital. I am now fortunate enough to be able to submit to the Section parts of the skull of this man, which exhibit a well-marked example of union of fracture of the frontal region, which extended by radiation to base.

The case is of interest because of the rarity of the pathological demonstration of union of fracture of the base of the skull, and for the lesson of caution which it teaches with regard to the indications for trephining for the treatment of epilepsy. The man was admitted to hospital in an epileptic fit, and between the time of his admission to the time of my seeing him—somewhere about twelve hours—he had over seventy attacks of convulsions. Only an imperfect history of his previous ailments had at the time been obtained, and the rapid succession of his convulsions, and of the following attacks of coma, made it quite impossible to get any information from the patient. His epilepsy was of the Jacksonian type, the convulsion starting in the left hand and arm. There was an uncertain account of previous head injury, and on the shaven scalp a long scar extended over the region of the motor area of the right side.

I was asked by my colleague, Dr. Walter Smith, to see the patient with the object of determining whether operative treatment was admissible. The patient was so evidently moribund we

decided in the negative, but I feel very certain that had the conditions been at all more favourable that I should have been much inclined in favour of operation. When we came to examine the skull, and also when we were able to get a clear history of the previous facts of the case, the error such proceeding would have involved was evident.

When the scalp was raised it was seen that no lesion of the external surface of the bone was to be found beneath the scar in the scalp. When the calvaria was removed the internal surface of the bone in this position was without visible defect, and the membranes and the surfaces of the cerebral convolutions were normal here also.

In cleaning the bone in order to remove the calvaria, we found at the glabella and the external extremities of the superciliary ridges, clear traces of old united fractures. Above this region, and inclined to the right of the mesian line, an area of bone, semicircular in shape, had been broken and slightly depressed. The radius of the circle, one half of which covered this region, is an inch and three-eighths. From the lower part the lines of fracture pass into either orbit, and converging meet in the cribriform plate of the ethmoid. Backwards from this a single fissure passes through the sphenoid, opening the roof of the sphenoidal sinus, and, going just internal to the optic foramen, is lost in the groove for the internal carotid artery on the body of the sphenoid. This last line of fracture is, in the macerated bone, wide open, without osseous union, and with its edges rounded by absorption. The only definite trace of brain injury was found in the convolutions of the right anterior lobe in the mesian fissure, in front of the corpus callosum—parts directly overlying the fissure I have described in the bone of the anterior cranial fossa. In this position the surface of the brain was stained a deep brown colour by the remains of incompletely absorbed blood.

Inquiry into the history of the case made it clear that the epilepsy was the prime disease.

Ten years before his death the man had broken his patella by falling in a fit from the driving-seat of his cab, and was at the time

under my own care for his fracture. About two years before his death he fell again from his cab in a fit, and was carried to Jervis-street Hospital, where he was treated for fracture of the skull.

His kidneys were extremely diseased, and during his short fatal illness his urine was found to be loaded with albumen.

A CONTRIBUTION TO THE PATHOLOGY OF JOINT BODIES.

By R. GLASGOW PATTESON, B.A., M.B., F.R.C.S.;
Assistant-Surgeon to St. Vincent's Hospital, Dublin.

[Read in the Section of Pathology, November 29, 1889.]

The obscurity which involves the pathology of loose bodies in joints is shown by the manifold theories which have from time to time been put forward to explain their occurrence. Putting aside "foreign" bodies, in the proper sense of bodies which have obtained entrance into a joint from external sources, and confining the term "joint bodies" to those developed in, or in connection with, a joint, the following classification embraces all the varieties I have been able to find described by the various authors who have written on this subject:[a]—

1. *Fibrinous Bodies.*—The result of hæmorrhage or of fibrinous inflammation in a joint: the so-called "melon-seed" bodies (Hunter).
2. *Lipomatous Bodies.*—The product of hyperplasia of the sub-synovial fatty tissue (Orth), such as occurs in the condition described as lipoma arborescens (J. Müller); or of hyperplastic increase and subsequent fatty degeneration of the villi of the synovial membrane.
3. *Fibrous Bodies.*—Resulting from papillary hyperplasia and detachment of the synovial villi; the papillary fibroma of Virchow.
4. *Fibro-cartilaginous Bodies.*—The product of hyperplasia of islets of cartilage cells occasionally found in the villi of the synovial fringes (Rokitansky).

[a] In this list I have, for the sake of brevity, omitted references to literature but have, as far as possible, indicated in brackets the names of the earliest describers of the various conditions.

5. *Cartilaginous Bodies.* — Due to detached ecchondroses in arthritis deformans (Laennec), or to traumatic separation of a portion of articular cartilage (Bloch).
6. *Osteo-cartilaginous Bodies.* — A further development of the cartilage cells of the villi of the synovial membrane (Rokitansky, Virchow), or a nodosity which has become detached in joints the seat of chronic rheumatic arthritis (Brodie, Adams, Cruveilhier).
7. *Osseous Bodies.*—The result of separation by fracture of an osteophyte (Billroth).
8. *Traumatic Bodies.*—Portions of articular cartilage with a layer of subjacent bone, which have become detached by direct violence, and set free in the cavity of the joint (Teale, Marsh, Jacobson, Kragelund).
9. *Necrotic Bodies.*—Which occur when portions of the articular surface, either purely cartilaginous or with a portion of subjacent bone, have become sequestrated by a process of "quiet necrosis," following a less degree of violence to the joint (Teale, Paget, Kragelund).

I am indebted to the kindness of my colleague, Mr. M'Ardle, for the opportunity of examining and describing the accompanying specimen, obtained from a young man aged 24. Eight years previously he first noticed "something in his knee." It could then be felt at the inner side of the ligamentum patellæ, and was about the "size of a small pea." It used occasionally to slip in between the articular surfaces, giving rise to the characteristic train of symptoms. There was no history of antecedent injury or of rheumatic pain. In this situation it gradually increased in size, and three years ago, when it had increased to the size of a bean, he dislodged it from this position himself, pushing it upwards into the joint in the hope of obtaining permanent relief. Disappointed in this, and the body still continuing in growth, he sought admission into St. Vincent's Hospital. The body was found lying in a pouch of the synovial membrane, above and to the inner side of the patella. The operation was performed a few days later by Mr. M'Ardle,

the body being displaced to the outer side of the joint, and removed by direct incision. The patient made an excellent recovery.

The body, on examination, was found to be of an irregularly oval shape, reminding one somewhat of a small patella. It presented one entirely cartilaginous surface, and one composed of a bony centre with a surrounding cartilaginous rim (fig. 1, A., B.). Its cartilaginous surface was smooth and glistening, showing only a few slight depressions; the bony surface was rough and irregular, and raised above the marginal collar of cartilage, which at one border was almost hidden by the overlying ridge of bone. It measured in length 28 mm., in breadth 22 mm., in thickness 11 mm. A median section showed it to be composed of an oval bony nucleus, covered on one side by a crescentic disc of cartilage (fig. 1, C.). The greatest thickness of bone was 8 mm., of cartilage 4 mm. The bone was of a yellowish colour, and of denser structure than normal spongy bone. At the point seen in section, the uncovered surface of bone was only slightly mammillated; but in other parts it presented a rough and uneven surface, the hollows being filled with fibrous tissue, while on the projecting bony points distinct evidences of eburnation could be seen. There was no hilum, and at no part could any traces of a pedicle be found.

On microscopic examination the cartilaginous surface was seen to be completely invested with an envelope of cellular fibrous tissue (fig. 2 A.), which also extended over the margins of the bony surface, filling the interstices between the trabeculæ, and dipping into, and at times completely filling, the neighbouring medullary spaces. Beneath this the cartilage presented all the characters of articular cartilage, except that cell-division was over-active in the intermediate or middle zone. Most superficially placed was a layer of cells lying in a homogeneous intercellular substance, and arranged parallel to the surface, the primary and secondary capsules being distinctly seen (fig. 2, A.). Below this lay a zone of irregularly-grouped round or oval cells, much broader than that found in ordinary articular cartilage. These cells possessed a distinct contour, a deeply-staining nucleus, and well-defined capsules, the matrix in which they lay being slightly

Fig. 1.

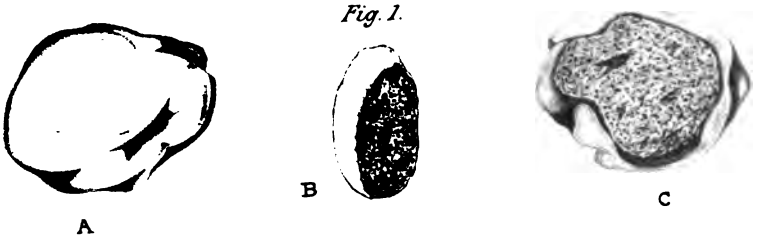
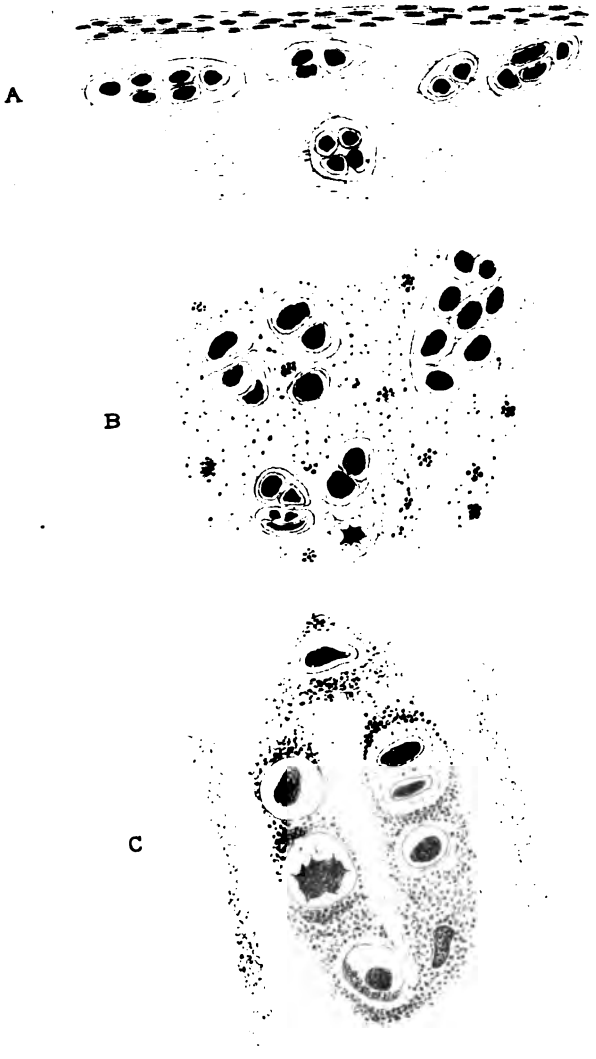


Fig. 2.



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F. Huth, Lith^r Edin^r

LOOSE BODIES IN JOINTS.

fibrillar and more or less infiltrated with lime-salts between the cell territories (fig. 2, B.). Beneath this layer, and arranged perpendicularly to the underlying bone, was a third, or osteogenetic layer, the cells of which had mainly lost their capsules, and lay embedded in a finely granular and, for the most part, calcified matrix (fig. 2, C.). In one or two limited spots the bony trabeculæ were undergoing a retrogressive metaplasia into a cartilaginous-like tissue.

In further considering this subject, it is my intention on the present occasion to deal with only two of the many questions of pathological interest suggested by the study of this body—firstly, as to its mode of origin; and secondly, as to its mode of growth.

I. *Mode of Origin.*—In this case we can absolutely exclude a traumatic causation, either direct or by a process of slow exfoliation. Although the possibility of traumatic causation is denied by Ollier,[a] and regarded by many others with scepticism, notably in these countries by Professor Humphry,[b] it can hardly be longer doubted after the numerous clinical instances which have been recorded both here and on the Continent, and which attest the reality of its occurrence.[c] Excluding traumatic causes, one thing is certain, that loose bodies can only occur in a joint more or less diseased, and often afford the only evidence of a pre-existing affection of the joint. Laennec regarded them as, in many cases, extra-articular formations, which resulted from recurring attacks of synovitis, and which subsequently found their way into the

[a] *Dict. Encyl. des Sciences Méd.*

[b] *Brit. Med. Journal*, September 29, 1888, vol. ii., p. 707.

[c] *Vide* Simon, *Path. Soc. Trans.*, vol. xv., p 206, 1865; Teale, *Brit. Med. Journal*, May 26, 1888, vol. i., p. 1109; Jacobson, *Operations of Surgery*, note on p. 1032; and the two cases in the article by MM. Poulet and Vaillard, subsequently referred to. Virchow, after adverting to the comparative infrequency of loose bodies of traumatic origin in healthy joints, refers to those cases in which correspondence in shape and size between the body and the lesion of the joint surface is so exact that it is hard to conceive it as a result of pressure-absorption and concludes—"Les faces se correspondent parfois si parfaitement et sont si profondes, que l'on peut à peine se refuser à admettre leur connexion primitive," *Traité des Tumeurs*, tome i., p. 454.

joint. If such an origin is possible, it must at least be one of very rare occurrence. The most generally accepted theory is that of their derivation from the synovial fringes. Since Kölliker demonstrated the occasional presence of cartilage cells in the villi of the synovial membrane, quite an exaggerated share has been allotted them in the pathology of joint diseases. Rokitansky[a] first put forward the theory of their causal relation to joint bodies of osteocartilaginous nature, to the exclusion of every other cause. To a certain extent his view has been adopted by Virchow[b] and following him the majority of writers have accepted the presence of these isolated cartilage cells as a circumstance which adequately accounted for all the varieties of joint bodies. Though no doubt exists that they do give rise to certain forms of bodies, and that every step from commencing development, through the stage of pedunculation, and so to complete freedom, may be at times observed in a single joint, yet we must remember that Kölliker himself admits that these cartilaginous centres occur but rarely, and subsequent observers have failed to find them in healthy joints. Accordingly we must not attribute to them an exaggerated *rôle* in the production of joint bodies, to the exclusion of other, at least equally probable, modes of origin.

Sir Benjamin Brodie[c] seems to have been the first to recognise the important part played by chronic rheumatic arthritis as a factor in the production of osteocartilaginous bodies. His observation is worth quoting. "In cases in which the joint has been long the seat of disease (especially of that which I have described under the name of rheumatic gout), it occasionally happens that a bony ridge is formed, like a small exostosis, round the margin of the articular cartilages. In the two cases to which I allude this preternatural growth of bone had taken place, and in consequence of the motion of the parts on each other, portions of it had been broken off and lay loose in the cavity of the joint." Cruveilhier regarded this as the most frequent mode of origin, but it is only of late that the full

[a] *Zeitschrift der Wiener Aerzte*, January 2, 1851.
[b] *Die Krank. Geschwülste*, Bd. i., Berlin, 1863.
[c] *Path. and Surg. Observations*, 5th ed., p. 261, 1850.

significance of the relationship has been determined.[a] This has been embodied in a most valuable and exhaustive article by MM. Poulet and Vaillard, based on an extensive series of pathological and experimental researches which throw a flood of light on this much-debated subject.[b] They examined six cases of pedunculated, but freely movable bodies, from joints affected with chronic rheumatic arthritis. All consisted of bone and cartilage, and all proved to be developed from osteophytes which had undergone varying degrees of separation. The similarity of their structure to that of two loose bodies, which originated after injuries—one in the elbow and one in the knee—led them to examine the mode of development of the osteophytes which are found in this diseased condition of joints. On examining them they found that there exists on the surface a primary fibrous envelope, which is continuous with the periosteum of the epiphysis on the one side and with the reflection of the synovial membrane on the other. In the centre of this projecting nodule of articular cartilage there is developed a bony nucleus, formed of trabeculæ of varying degrees of density, which are more or less in continuity with the cancellous tissue of the epiphysis by means of slender processes of bone which traverse the cartilaginous neck of the osteophyte. Should fracture or pathological separation occur through this neck, the detached fragment will, after partial absorption of its base, and extension of the fibrous capsule over it, present all the characters of an osteo-cartilaginous joint body. In this way the authors account for the appearance of these highly-organised bodies shortly after the occurrence of an injury to a joint, as it is no uncommon occurrence

[a] "Ils ne sont autre chose que des végétations, des ostéophytes, des ostéochondrophytes, des chondrophytes, ordinairement sphéroïdaux, toujours lisses lorsqu'ils sont cartilagineux, lisses ou tuberculeux lorsqu'ils sont osseux. . . . . Dans le plus grand nombre des cas, ils ne sont autre chose qu'une végétation ou production osseuse née du pourtour des surfaces articulaires," Cruveilhier, *Traité d'Anat. Pathol.*, tome ii., pp. 135, 136, Paris, 1852.

[b] "Contrib. à l'étude des Corps étrangers ostéocartilagineux et osseux des Articulations," par MM. Poulet et Vaillard, *Archives de Physiol.*, v., 1885. This article seems to have been strangely overlooked, and to it I am largely indebted for the foregoing facts and statements on which subsequent conclusions are based.

for such a body to be found immediately after the subsidence of the accompanying synovitis. In order to compare the changes undergone by portions of articular cartilage and bone in healthy joints after mechanical separation, they undertook experimental observations on animals. The elbow-joint was opened, and a small portion of the articular cartilage and its underlying bone chipped off with mallet and chisel from the head of the radius. The detached fragment was fixed in the anterior part of the joint, and the wound closed with strict antiseptic precautions. Observations were made on three dogs after a lapse of 37, 43, and 123 days respectively from the time of operation. In all cases it was found that adhesions had been formed with the synovial membrane, in one case by the whole extent of the bony surface. Alterations, indicating active progress in the direction of absorption, were found in every case—fibrous transformation of the cartilage and medulla, and gradual destruction of the bony framework by direct fibrous metaplasia, by pericorpuscular absorption, and by the lacunar erosion of Howship. These appearances were in striking contrast to those noticed in the free bodies, which, living only by imbibition, showed uniformly retrogressive changes of tissue in the direction of a more embryonic type, such as transformation of bone and medulla into cartilaginous tissue. But it is possible that in their experimental observations these active changes may have been determined by inflammatory reaction in the joint, so that too much importance should not be attached to them. But their observations show conclusively the identity of structure which exists between "loose bodies" and wholly or partially attached osteophytes in rheumatic arthritis.

In order to determine the causation of loose bodies, Kragelund examined the joints from thirty cases.[a] In eighteen he found well-marked signs of chronic rheumatic arthritis, and he regards this condition and traumatism as the chief factors in their causation. He recognises two forms of traumatic bodies: those resulting directly from the injury, and those produced, after a longer or

[a] Kragefund, "Graduation Thesis," Copenhagen, 1886. Quoted in *Virchow und Hirsch's Jahresbericht*, 1887.

shorter interval, by a process of localised necrosis determined by a limited inflammation (*Quiet Necrosis* of Paget).

Taking, then, all these facts into consideration, in conjunction with the microscopic characters of the specimen described above, I have no hesitation in regarding it as a product of chronic rheumatic arthritis. Objection may be made to the age of the patient and the absence of previous symptoms. MM. Poulet and Vaillard direct special attention to the latent character of this variety of the disease which will not surprise any one acquainted with its Protean peculiarities. "In contradistinction to the common rheumatic arthritis," they remark, "it is a variety which may occur in young subjects, may be limited to a single articulation, and localised to a single spot in it."[a] In the present case, the eburnation of the exposed bony surface, the limiting fibrous capsule derived from the reflection of the synovial membrane, the marked proliferation of the middle zone of cartilage, some of the capsules containing eighteen or twenty cells, as well as the pathological changes exhibited (though in slight degree) by the medulla and bone, all agree with this theory of its origin. No other hypothesis affords at all as satisfactory a solution of the difficulty.

II. *Mode of Growth.*—I shall now only briefly allude to this, the second question which suggested itself for consideration. In the case of pedunculated or partially-separated bodies, or in those which having been free have again formed connections with the synovial membrane, no difficulty presents itself—they draw their supplies from the general circulation. But that bodies dependent for all supply on the synovial fluid should not only preserve their vitality but show signs of active increase, has been regarded by many with incredulity, and has led to very opposing views. Sir Astley Cooper absolutely denied the possibility of its occurrence; his contemporary, Russell, as strongly affirmed it. Von Recklinghausen[b] accepts the theory of their increase in size; and Virchow, after exposing the fallacy which may attend the sudden conglomeration of several small bodies, thus simulating the rapid growth of

[a] *Vide* also Virchow, *Traité des Tumeurs*, tome i., p. 457.
[b] *De corpor. lib. Articulorum*, 1864.

one, adds—"Theoretically, one cannot oppose any objection to the persistence of a lowered, if not an active, vitality in the detached portions of cartilage or bone. Not only can they obtain from the synovial fluid materials sufficient for their nutrition, but they can also increase in size."[a] The majority of French writers refuse to accept the theory. Poncet says—"Everything tends to prove a slow but gradual resorption of the arthrophytes; nothing justifies us in concluding that they can increase by proliferation of their elements."[b] But that this does not always hold good is proved by the case before us. We are fortunate in having a definite history, extending over a period of eight years. Of the first five years of its existence we can affirm nothing; but it is probable that, as it lay beside the ligamentum patellæ, it was attached either to the bone or by secondary adhesion to the synovial membrane, and was drawing its supplies from the blood. But when, three years ago, the patient pushed it up into the joint, that attachment must have been ruptured to allow of its travelling so far. It was then the size of a bean. At what exact time it found a resting-place in the supra-patellar pouch we do not know, but it had lain there for a considerable time, and continued to increase in size until it was removed, yet no trace of any attachment or of a pedicle could be seen. Here is evidence not merely of preserved vitality, but of fairly active growth—in three years it has at least doubled in size. Microscopic examination also confirms this, and disproves the accuracy of M. Poncet's statement. The cells show a distinct

[a] And, in further support of its probability, he adds, "Car, dans le fait, les pédicules, en tant en général qu'ils ne sont pas détachés, sont aussi *ordinairement peu vascularisés*, quoiqu'ils ne manquent pas toujours de vaisseaux, *et cependant les corps s'accroissent*" (Virchow, *loc. cit.*, i, p. 459). And Billroth, in a passage bearing on this point, remarks: "Die viel häufigere Art der Entstehung von Gelenkmäusen ist aber die, dass sich in der Gelenkkapsel dicht unter der Synovial-membran verknöchernde Knörpelkörper (Osteophyten) bilden, welche sich ins Gelenk hineinstülpen, die Synovialis vor sich herschieben, stielartig ausziehen, die schliesslich aber abreissen und frei werden können. Wahrscheinlich wächst der frei in Gelenke liegende Körper nicht mehr; *undenkbar wäre es freilich nicht, dass er sein Ernährungsmaterial aus der Synovia zöge*" (*Allgem. Chirurg. Patholog. u. Therapie*, Billroth u. von. Winiwarter, s. 702, Berlin, 1887).

[b] *Revue de Chirurgie*, 1888, p. 819.

contour, possess a sharply-defined nucleus, and in their reaction to staining agents behave as living cells. Moreover, in many parts they are in a state of undoubtedly active proliferation. These characters do not belong to enfeebled or merely existing tissues—they are peculiar to living, active growth.

To summarise the conclusions based upon an examination of the foregoing facts:—

1. In the majority of cases, loose bodies of mixed cartilaginous and bony nature are *wandering osteophytes*, and are evidences of a rheumatic arthritis, either manifest or latent.
2. This form of chronic rheumatic arthritis is insidious and latent in many instances, occurs in young persons as well as in those more advanced in years, is often limited to a single joint, and is chiefly manifested in "lipping" of the margins of the articular cartilages, and in the formation of ecchondroses and osteophytes, which while attached cause no symptoms.
3. These bodies, after liberation in the joint, may, and do often, undergo considerable increase in size, living by imbibition, and their nutriment being derived from the synovial fluid. This fact points to the necessity of their early removal by operative interference.

EXHIBIT OF MICRO-ORGANISMS, WITH SOME REMARKS.

By EDMOND J. McWEENEY, M.A., M.B., &c.;
Pathologist, Mater Misericordiæ Hospital; Lecturer on Pathology, Catholic University Medical School.

[Read in the Section of Pathology, January 10, 1890.]

In laying the following pure cultures before the Academy to-night, I shall confine myself to the briefest possible allusion to each, They have all been made by me when working last November in the Laboratory of Dr. Robert Koch at the Hygiene Institute of the Royal University of Berlin.

The following organisms were then shown, and a few words said about each:—

Non-Pathogenic Organisms:—

Bacillus subtilis (*Ausstrich*-culture on agar-agar)
„ megaterium „ „ „ „
„ prodigiosus (on potato)
„ fluorescens (*Stich*-culture in gelatine)
Spirillum rubrum (in an Esmarch's tube)

Pathogenic Organisms:—

Bacillus anthracis (*Ausstrich* on agar-agar)
„ mallei (glanders) (*Ausstrich:* also on potato)
„ tuberculosis (*Ausstrich* on glycerine agar)
„ pneumoniæ crouposæ (Friedländer) (*Ausstrich*)
„ pyocyaneus (*Stich* in gelatine which had liquefied)
„ of swine-erysipelas (*Stich* in gelatine)
„ Typhosus (on potato)
„ Cholera Asiatica (Koch) (*Ausstrich*)
Micrococcus tetragonus (*Stich* in gelatine)

In explanation of the not very typical character presented by some of the cultures, I may say that regard has necessarily been paid, in the selection of the substratum, rather to the portability of the specimen than to securing the most typical appearance. It is this circumstance which has caused me to select oblique (Schräg) agar-agar in so many cases. All the cultures are pure with the exception of that of the typhoid bacilli on potato, which has become contaminated with a spot of the common yellow sarcina of air. I have come provided with microscopic slides illustrative of each species, but the microscopic accommodation at the disposal of exhibitors permits me to show only two objects—viz., a so-called "Klatsch" preparation of the anthrax bacillus showing its peculiar method of growth, and a slide of a very interesting organism, cultures of which I am unfortunately not in a position to lay before the Section—the Nicolaier-Rosenbach *bacillus tetani.* The chief biological points in regard to this organism have been lately worked out in detail by a Japanese investigator, Kitasato, in Dr. Koch's Laboratory. He found it strictly anaerobic, gas-forming, and capable of secreting a peculiar pigment. Its pathogenicity seems, according to the most recent researches, notably Hœgler's [quoted in *Centralbl. f. Pathologie* for Jan., 1890], to be firmly established. My specimens show its peculiar method of spore formation, by which it is at once distinguished from allied species. On one, sometimes both ends of the rod, a spherical protuberance makes its appearance, which becomes detached, and possesses the resistance against external influences characteristic of spores. When in this condition the organisms closely resemble minute, round-headed pins. The establishment of pure cultures of this organism is at all times a matter of considerable difficulty. As this is the first occasion on which specimens of this remarkable bacillus—which acts by means of its *ptomaine* named tetanotoxin by Brieger, who isolated it—have been exhibited in Dublin, I trust the Academy will excuse the length at which I have referred to it.

ON THE PATHOLOGY OF EMPYEMA.

By HENRY T. BEWLEY, M.D., F.R.C.P.;
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Lecturer on Medical Jurisprudence in Trinity College, Dublin.

[Read in the Section of Pathology, January 10, 1890.]

Acute suppuration we may, I think, consider as being in almost every case due to the action of micro-organisms. Professor Watson Cheyne, in his Lectures on Suppuration and Septic Diseases, can only mention two affections in man in the pus of which competent observers have failed to detect bacteria. In some cases of suppurating bubo after soft chancre no organisms could be found, especially in those cases in which the bubo after being opened took on a chancrous appearance. Occasionally, too, under sal alembroth dressings, when the discharge from the wound is free, sores or pustules form on the skin which contain a sticky semi-purulent material, but no bacteria. "With these exceptions," Cheyne says, "we always find that suppurations occurring naturally are associated with micro-organisms."

We may, therefore, consider that empyema is due to the entrance of pus-producing organisms into the pleural cavity. These micro-organisms have been looked for and found by a large number of observers. On the other hand, serous pleural effusions are usually free from organisms. Professor Ehrlich[a] states that "serous effusions are free from micro-organisms," and Professor Fränkel[b] stated that if micro-organisms are found in serous fluid removed by exploratory puncture from a pleural effusion, in these cases an empyema really exists already, the pus corpuscles, through their higher specific gravity, having sunk to the bottom, and left a layer of serum above.

[a] Berliner Klinische Wochenschrift. 1888. P. 408.
[b] Idem. P. 407.

The question as to the pathology of empyema may therefore be put in the following way:—What are the micro-organisms that cause empyema, and how do they get into the pleural cavity?

According to their pathology we may divide up cases of empyema into several classes. The manner in which the bacteria make their way into the pleural sac is in some cases plain and evident; in other cases it is more doubtful and difficult to be certain about.

(1) There are some cases of empyema which are due to the introduction of ordinary pus-producing organisms, either through the chest-wall or from the lung into a healthy pleural sac, or into a previously existing serous effusion; as when a penetrating wound of the chest-wall is received, when a pulmonary abscess or an area of gangrene in the lung comes to communicate with the pleural cavity, or when a septic exploring needle is introduced into a serous effusion.

(2) Some cases of empyema occur in immediate connection with croupous pneumonia, and are caused by the organism which is believed to cause many cases of pneumonia—the so-called pneumococcus of Fränkel. Out of 12 cases of empyema which this observer examined,[a] in three he could not find in the pus any other organism except the pneumococcus. He remarks that he has found the prognosis in cases of empyema due to the pneumococcus to be better than in those which are caused by the ordinary pus-producing micrococci.

As to the exact route which the pneumococci take in making their way from the lung into the pleura—whether they make their way directly through the pulmonary pleura, or whether they get into the blood and lymph, and are so carried to the pleural sac—I do not know of any evidence to enable us to form any definite opinion.

(3) A third group consists of those cases in which the infective virus is the tubercle bacillus. In these cases tuberculosis of the lung exists, and the bacilli make their way through a hole in the pulmonary pleura. In some cases the rent in the lung is manifest and large, as when a phthisical cavity bursts into the pleural sac,

[a] Berl. Klin. Woch. May 14, 1888.

giving rise to a pyopneumothorax; in other cases minute perforations occur in the lung, through which a minute particle of tubercular matter gets into the pleural sac. In these latter cases a tuberculous empyema develops without any pneumothorax.

In some of these tubercular empyemas tubercle bacilli are few in number and difficult to find. Owing to this difficulty in finding tubercle bacilli, and the ease with which other organisms are found if present, Professor Fränkel[a] lays down the rule that if you fail to find any micro-organisms in an empyema it is probably tubercular in nature.

(4) In the next class which we have to consider the mode in which the pus-producing organisms make their way into the pleural sac is more obscure. I mean in those cases in which a serous effusion without any apparent cause becomes purulent. The germs in these cases cannot make their way in through the chest walls; there is no disease or opening through which they could get in. Again, I do not think it is likely that in these cases the virus makes its way from the lung through the pulmonary pleura. We have no reason for suspecting any breach of continuity in this membrane. It is much more probable—and the analogy of similar infective diseases supports this view—that the organisms get into the pleural sac by way of the blood and lymph vessels. Let us consider first the relations which exist between the tissues of an animal's body and these pathogenic germs, streptococci and staphylococci, organisms which are the cause of many serious and often fatal diseases—abscesses, pyæmia, cerebro-spinal meningitis, osteomyelitis, and many others. These organisms, if introduced into the blood of a healthy animal, often disappear in a very short time, being destroyed in the body or excreted by some of the excretory glands, or at any rate got rid of in some way, without causing any mischief. Such experiments have been frequently made on animals. In the case of human beings also pus-producing organisms have frequently been found floating about in the blood and not doing any harm. Von Eiselsberg[b] demonstrated the presence of staphylo-

[a] Berl. Klin. Woch. 1888. P. 407.
[b] Langenbeck's Archiv. Vol. XXXV.

cocci and streptococci, most frequently of staphylococcus pyogenes albus, in the blood of patients who were suffering from septic fever, and yet no abscesses formed. Children have been born suffering from osteomyelitis,[a] while the mothers, through whose blood the organisms must have come, remained in perfectly good health. Fränkel and Freudenberg[b] have found streptococcus pyogenes in the blood of many persons who were suffering from scarlatina, and no abscesses or septic affection resulted.

So that we see that these pus-producing organisms may exist in the tissues without doing any harm, and that they probably do so exist when the vitality of the tissues is not very good—*i.e.*, in depressed states.

But if one spot in an animal's body is especially weak, owing to injury, inflammation, or some other evil influence, then the case is very different. The natural forces, whatever they are, by which the body destroys these micro-organisms are unable to act in this weakened spot; the bacteria flourish, multiply, and produce their baneful effects.

If a number of pyogenic micrococci be injected into the blood-vessels of an animal, and at the same time one of his bones be fractured, the animal generally dies in some days, and the bone at the seat of the fracture is found in the condition of osteomyelitis. The tissues being weakened at the seat of injury, the germs were able to multiply there, and produce the disease.

Again, Wyssoskowitsch[c] and others, by injuring the tricuspid valves by means of a rod introduced through the jugular vein and then injecting micrococci into the blood, succeeded in producing typical case of ulcerative endocarditis. The injury led to a loss of resisting power.

Let us suppose the animal into whose blood the micrococci were injected, instead of having a broken bone or a torn tricuspid valve, had had a pleurisy with serous effusion, the cocci would probably

[a] Rosenbach, Micro-organismen bei d. Wund-infections-krankheiten de Menschen. Wiesbaden. 1884.

[b] Centralblatt f. Clin. Med. 1885.

[c] Zeitschrift f. Hygiene. Vol. I., No. 1. Virchow's Archiv. Vol. CIII.

have found the inflamed pleura and the effusion a suitable ground to develop in, and an empyema would have been the result.

Such, then, is the way in which we must explain these cases of empyema—namely, that under various conditions, especially when the general health is not good, different kinds of pathogenic organisms find their way into the tissues of the body, are carried about by the blood and lymph, and, if they find in any part of the body conditions suitable for their growth, develop and multiply, and cause suppuration.

There are other diseases whose pathology is in some respects comparable to that of empyema—namely, amongst others, ulcerative endocarditis, cerebro-spinal meningitis, and many deep-seated abscesses. As an instance of the last Watson Cheyne[a] mentions the case of a drunkard who had albuminuria. Whenever he received a bruise he developed an abscess at that part. In his depressed state the pus-producing organisms were able to live in his blood; and the injuries, by setting up the early stage of inflammation, and thus further weakening the tissues, and also by leading to the effusion of blood containing the organisms, gave rise to the formation of abscesses.

With regard to the question as to how they make their way into the blood and tissues in the absence of wound or injury, no definite answer can be given; but it seems probable that in some cases at any rate the germs make their way in through the walls of the digestive canal. Kocher[b] made experiments on dogs; he fed them on quantities of putrid material, and then found that an injury to a bone was followed by osteomyelitis. As a result of the putrid food bacteria increased in numbers in the digestive canal, and some of them succeeded in making their way into the blood—hence the suppuration after the injury. In other cases the infection may occur in other ways. We often cannot tell how the germs have got in; but "in all cases of internal suppuration," says Professor Weigert, "we must believe that a communication between the

[a] Brit. Med. Jour. 1888. No. 1418. P. 456.
[b] Deutsche Zeitschr. f. Chirurgie. Vol. XI. 1879.

seat of disease and the outside of the body exists, or has existed."*

(5) A fifth variety of empyema occurs in pyæmia. In this disease enormous numbers of micro-organisms exist in the body; they form emboli, which block up small blood-vessels, and give rise to secondary abscesses. The pleura may be affected, and a pyæmic empyema ensue. These cases differ from the last-mentioned, in that in these pyæmic cases the organisms probably exist in the blood in far greater numbers, and possibly possess greater virulence. Hence, they are able to overcome the resistance of the tissues and cause suppuration at almost any point, and do not need, as in the former case, to wait until, so to speak, some injury or inflammation opens the door for them.

To sum up my main conclusions:—

(1) Empyema is always caused by micro-organisms.

(2) These are of different species, there being no one specific variety.

(3) In some cases ordinary pus-producing and putrefactive bacteria get into the pleural cavity through some opening in the chest-wall or lung.

(4) Some cases are associated with croupous pneumonia and are caused by the pneumococcus.

(5) Some cases are due to the action of the tubercle bacillus.

(6) In some cases pus-producing micrococci, which have in some way got into the blood, but are not, if unassisted, able to develop in the body, find in an inflamed pleura or serous effusion a locality suitable for their development, and under their influence a serous effusion becomes purulent.

(7) Some cases are pyæmic.

* Eulenberg's Real Encyclopädie d. Gesammten Heilkunde. 2 Aufl. Bd. 6. "Enzündung." P. 348.

A CASE OF CORNEAL TUMOUR (FIBROMA?).

By ARTHUR H. BENSON, F.R.C.S.;
Ophthalmic and Aural Surgeon to the City of Dublin Hospital;
Assistant-Surgeon to St. Mark's Ophthalmic Hospital.

[Read in the Section of Pathology, February 14, 1890.]

FIBROMATA of the cornea are of such rarity that few of the ordinary text-books do more than mention their existence. Most of them indeed mention the disease in order to express their doubts of its ever existing, whilst very few give any description of it at all.

Berry (of Edinburgh), one of the very few who appear to believe in their reality at all, in his admirable text-book, recently published, gives a three-line description of corneal fibromata, which exactly (as if taken from it) corresponds with the account of a case reported by me to this Section in 1886, and published in Vol. V. of the Transactions, pp. 250 and 287.

Berry says (p. 90), speaking of fibroma of the cornea—"This is an exceedingly rare affection. It occurs as a flat growth of densely white appearance, involving the superficial portion of the cornea, over the whole of which it very slowly spreads."

Alt, in his "Histology of the Eye," alludes to corneal fibromata, but expresses his disbelief in their existence unless springing from the limbus conjunctivæ.

In the case which I now bring forward this description, however, does not suit:—

CASE.—J. R., aged seventy-two (*vide* 3,048 of S. M. O. H. Disp., Oct. 3rd, 1889), came to St. Mark's Ophthalmic Hospital, out-patient department, in October last.

He stated that be had been blind of his left eye for many years; that a few days before he had knocked his blind eye against something, and had since suffered intense pain from it. The other eye was normal.

On examining his eye I found that it was absolutely blind. L. V= 0—the tension of the globe was + 3 (old chronic glaucoma).

The conjunctiva was congested, and had the large tortuous vessels on it associated with chronic high tension.

The cornea was anæsthetic and very hazy, and adherent to its surface was a flat, smooth, nearly circular biconvex-lens-shaped growth of a lustreless gray colour.

It suggested the idea of a half-dried, small, cataractous lens, which had, by some accident, been only half removed from the eye through a corneal incision a little above the median line.

It was attached along its thin edges and hung down over the cornea, and was nearly, though not quite, covered by the upper eyelid.

It was freely movable except at its attachment, and seemed a tough something, adherent to, and covered by the corneal epithelium.

It measured about 5 mm. in its longest diameter, 2 mm. at its thickest part, and was nearly circular.

It was attached to the surface of the cornea above the centre by about 2 mm. or 3 mm. of its edge. There was no evidence of any recent injury to the cornea, nor was there any marked inflammatory condition observable anywhere.

The patient stated that he had never, until he got the knock on his eye a few days before, noticed anything on the eye at all. Immediately after the blow he noticed this growth, and he was positive that the blow caused it, and that it caused the pain.

I removed the growth, dissecting it with great ease off the cornea, to which it had but a very slender attachment. The relief to the man was (he said) immediate and complete, and I saw him several times afterwards. The ulcer that was left healed quickly, and no deep scar resulted.

My first idea was that possibly the case was one of corneal rupture, and extrusion of the lens. This idea has become untenable, and as it was the only one which seemed possible in view of the history which the man gave, I have been forced to consider the man's account of its origin as unreliable. Such a growth as this I had never before seen, nor can I find mention of such.

Horny growths of sorts are not uncommon on the cornea, but this has no similitude or relationship with them.

Dr. Graves (who kindly examined its structure) tells me it is histologically a fibroma; but how did it grow in so curious a shape; how long must a fibroma of that size have taken to grow; and how came it that its wearer was not made acquainted with its presence until the blow called attention to his blind eye? These questions are hard to answer.

Dr. Graves reports as follows:—"The growth, hardened in Müller's fluid, and cut in celloidin, appears to be a fibroma; it is surrounded by corneal epithelium except where it was cut off from the cornea.

"It consists of fibrous tissue, with blood-vessels here and there in it. At the thickest end, which is the end most remote from the cornea, it somewhat resembles corneal tissue, with the fibres separated by small round cells; many of these cells are blood cells.

"The thin end is also fibrous, and the cut ends of the fibres are very distinct where it was cut away from the cornea.

"If you take the view that it is an eccentric hypertrophy of the corneal tissue, perhaps it would be better to call it a keratoma; but the fibres are thicker and less wavy than corneal fibres, and it has blood vessels in it, which, of course, the normal cornea has not.

"Whatever view may be taken of this growth, it seems clear, from the description given, that it started from the cornea—not from the limbus conjunctivæ.

"Besides the cases already alluded to, Mr. Story and Mr. Scott (Trans. Acad. Med., 1888, p. 376) describe a corneal fibroma which replaced the corneal tissue; this they looked upon as probably congenital."

The photographs, which were kindly taken for me by Dr. Arthur Baker, show fairly well the microscopic appearance of the growth.

I do not think that in the consideration of the case the patient's own account of its origin need be given much, if any, weight. So negligent are people about their immediate concerns that many a growth of more direct injury than this tumour, hanging from his

blind eye, has escaped the observation of its owner until some external circumstance directed attention to the part affected.

This tumour must, I take it, have been of long standing and slow growth, and the eye being blind, and the man not very observant, it escaped detection till the blow forced his attention to the eye.

But the history is not the real puzzle. The rarity of corneal tumours of all kinds makes the existence of such a growth in such a situation a conundrum, the explanation of which I shall not attempt.

A CASE OF ADENO-CARCINOMA OF THE LOWER JAW, WITH PECULIAR METASTATIC DEPOSIT.

By EDMOND J. M'WEENEY, M.A., M.B.;
Pathologist, Mater Misericordiæ Hospital; Lecturer on Pathology, Catholic University School of Medicine.

[Read in the Pathological Section, Feb. 14th, 1890.]

The case, the details of which I have the honour of laying before the Academy this evening, seems to me worthy of notice on account of—

1st. The unusual occurrence of a tumour of such microscopic structure in connection with the lower jaw.

2nd. The peculiar situation of the only secondary deposit found, and

3rd. The divergence between the primary and the secondary growth in microscopic structure, presenting a good example of carcinomatous change in the metastatic deposit from a purely adenomatous tumour.

Case.—The patient, J. S., was a man, aged about seventy, admitted into the Mater Misericordiæ Hospital, October, 1889. The tumour sprang from the gum apparently by a broad base, just behind the lateral incisor on the right side. Here it formed a mass, about the size of a pigeon's egg, displacing the lateral incisor, which was carried altogether out of the alveolus and was supported on the tumour itself, to which it adhered by the gum, which was involved in the neoplasm. The consistency of the tumour was somewhat hard; it was covered with mucous membrane, and nowhere ulcerated. It had made its appearance about a year previously, and grown rapidly. The gums on each side of the tumour appeared thickened and infiltrated for a considerable area. Mr. Coppinger, under whose care the patient was, removed the

front part of the lower jaw as far back on each side as it seemed affected—viz., as far as the first bicuspid; but no reaction was established, and the patient rapidly sank, dying twenty-four hours after the operation.

The *post mortem* notes are as follows:—

Autopsy twelve hours after death. Body emaciated; slight general icterus; rigor mortis extremely distinct. On opening the mouth the front of the lower jaw, as far back as the bicuspids on each side was wanting. No sign of reaction. Nodules somewhat similar in appearance to the tumour could be seen and felt beneath the tongue in the floor of the mouth.

Larynx and *trachea*, *pharynx* and *œsophagus* healthy; *lungs*, emphysematous; *pleura* generally adherent on right side.

Heart.—Slight mitral stenosis.

Abdomen.—The sigmoid flexure of the colon took a remarkable course. The descending colon having arrived at the brim of the pelvis, turned sharply upwards and inwards, rising to the level of the ensiform cartilage. It then turned downwards again, keeping to the right of the middle line, as far as the prominence of the sacrum, when it assumed a mesial position, and was continued as far as the rectum to the pelvic outlet. The part of the gut attached to it was enormously distended with gas, and occupied nearly the whole of the front of the abdominal cavity.

Spleen.—Normal.

Kidneys.—Small, tough, fibroid.

Adrenals.—Normal.

Ureters and bladder.—Normal.

Liver.—Small, pale; very firmly fixed. On cutting into it *in situ*, considerable quantities of grumous brownish-yellow material exuded from orifices of bile ducts. Liver substance friable; colour pale. Gall-bladder very tense, dead white, matted together very closely with duodenum just outside pylorus, and hepatic flexure of colon, the whole constituting a dense white mass, the structures being cemented together by tough, evidently neoplastic, tissue. The walls of the gall-bladder were greatly thickened, and the contents were colourless and mucoid. The cystic duct was

obliterated, ending blindly in the neoplasm, which was here very dense, creaking like scirrhus on being cut, and presenting numerous points of fatty degeneration. The liver itself was slightly invaded by the neoplasm in the neighbourhood of the portal fissure.

Common Bile Duct.—Wide enough to allow little finger to pass through it. It perforated the neoplasm and opened into the duodenum by means of an aperture nearly as large as a threepenny bit.

Pancreas.—Head increased in size, owing to invasion by the neoplasm; distinct boundary line between the tumour and the pancreatic tissue.

The lymphatics about cœliac axis were infiltrated, and the vena cava and hepatic artery were also involved, but their lumina not encroached on.

Stomach and intestines healthy. The hepatic flexure of the colon and the commencement of the duodenum were fixed by the growth, but the mucous membrane was nowhere implicated.

On microscopic examination the tumour of the jaw and that in the pancreatic region were found to be similar in structure, with certain points of difference, which will be alluded to later on. They consisted of well-formed gland tubules which were lined with very long columnar epithelium and scattered through a connective-tissue stroma, studded with round and spindle-shaped nuclei.

Many of the tubules in the tumour of the jaw had their lumina filled up with material which looked like secretion. It was composed of structureless substance, thickly studded with small compound granules, and it quite distended some of the tubules.

A distinct basement membrane, with flattened nuclei was traceable, and the columnar cells were disposed on it with perfect regularity. The structure of the pancreatic tumour was somewhat similar; but the lumina of the tubules were much less distinct, and the connective tissue stroma more abundant.

As regards the relation of these two neoplastic masses to one another, the writer is disposed to look upon the jaw tumour as the primary, the other being merely a metastatic, deposit. It must be conceded that the occurrence of such a metastasis is unusual,

and it may, perhaps, be well to give the reasons which inclined the writer to this view.

The jaw tumour is strictly regular in structure as regards the tubules. Nowhere is the basement membrane broken through by epithelial proliferation. The outline of the tubes is regular, close-packed as they are, and the lumen is distinct, whereas in the abdominal growth many of the tubules are converted into mere islets of epithelium, solid strands of the most irregular shape, ramifying through the stroma. In the neighbourhood of the pancreas the transition was beautifully gradual from the most typically glandular to the most atypical (carcinomatous) structure. It seems to me more probable that an adenomatous growth should give rise to (partly) carcinomatous metastases than that the reverse could be the case. Of course, the fact must not be overlooked that the bile ducts are very apt—much more so than the mucous membrane covering the gums (where the occurrence of normal gland tissue is doubtful)—to give origin to adenocarcinomatous growths. But a solitary mestastasis from bile ducts to the gums and floor of the mouth would be very unusual indeed—much more so than a mestastasis from mouth to bile ducts or liver. No trace of any tumour could be discovered in any part of the intestinal tract, though careful search was made, especially in the rectum. The case seems to me to possess several points of interest, which have induced me to bring it before the Pathological Section of the Academy of Medicine.

TUMOUR OF THE BRAIN, ATROPHY OF RIGHT KIDNEY, WITH HYPERTROPHY OF THE LEFT KIDNEY.

By JOSEPH REDMOND, M.D., F.R.C.P.;
Physician to the Mater Misericordiæ Hospital;
Senior Physician to Cork-street Hospital.

[Read in the Section of Pathology, March 21, 1890.]

The specimens which I have the honour of exhibiting to-night before the Pathological Section were removed from the body of a patient, aged twenty-nine, who died in the Mater Misericordiæ Hospital on the 30th April, 1889. The details of the case may be of some interest on account of the large amount of attention that is being devoted at the present time to diseases and injuries of the brain. The notes of the case are as follows:—

Case.—J. W., aged twenty-nine, a clerk by occupation, was admitted into the Mater Misericordiæ Hospital on the 29th April, 1889. His family history was as follows:—His father died of aneurysm, and a brother had recently died of peritonitis; his mother and other members of his family were living and healthy. He stated that he had been previously in the hospital under treatment for some urethral trouble, his stay in hospital on that occasion lasting from the 21st December to the 12th January. The patient was of temperate habits, and attributes his present illness to a blow which he received while attending a meeting towards the end of December, 1888. He stated that on that occasion he received a severe blow of a baton over the right ear, which, he said, did not completely stun him, but, as he described it himself, he was so much overpowered that it was with difficulty he succeeded in making his way home. From that time until his death he complained of severe pain in the right side of his head and suffered from fits, which usually occurred on Tuesday in each week. He attributed the regular recurrence of these fits on Tuesdays to the

fact that Monday was always a busy day with him, his occupation being that of a railway clerk. When in a fit he says that he resembled a drunken man, so much so that on one occasion he was arrested by the police for being intoxicated, and confined in the cells for twelve hours. He states that he knows well when a fit is about to come on, and that during the attack, although he is apparently unconscious, yet he is quite well aware of all that is passing around him. The seizures, as observed in the hospital, were not ushered in by any cry or moan—the patient simply sank into an apparently semi-conscious condition. There was no frothing at the mouth, nor was there any attempt at closing the teeth; there were no convulsive movements of the muscles of either the face or limbs; the pupils were normal, but slight pallor of the face was observed; when spoken to. the patient would not answer. These attacks lasted from about 10 to 15 minutes, and during this time the patient usually lay on his back, with his hands clasping the sides of his head, and from time to time would give vent to slow and deep moaning cries, as if he was suffering great pain. On the evening of his death he had several such seizures following one another in rapid succession, during one of which he expired, his pulse a few moments before death being 90, heart action regular.

Post mortem (which was made by Dr. M'Weeney 52 hours after death)—Rigor mortis well marked. Body that of a well-nourished, muscular man.

On removing the skull cap the posterior part of the right parietal bone presented a very slight depression on its inner aspect, about the size of a shilling, surrounded by a slightly elevated reddish ring of bone. The dura mater was but loosely connected to the bone except at the above-mentioned spot, where it was rather closely adherent. The part of the membrane corresponding to the spot was opaque and white. On removing the dura mater it was found to be detachable from the pia mater with difficulty at this point, and this operation required the exercise of considerable care. The pia mater everywhere was deeply congested, and at the above-mentioned point it was elevated slightly and white.

Brain remarkably soft. At the spot in question a tumour about the size of a pigeon's egg extended into the surface of the hemisphere. It was wedge-shaped, with thin edge inwards, and occu-

ed the region of the angular and portion of the supra-marginal yri. It was hard, and yellowish-white in colour. The surrounding brain substance had undergone white softening, so as to be almost diffluent. The rest of the brain seemed healthy enough. The ventricles, especially the left lateral, contained a considerable amount of serum.

Thorax—Both lungs were very deeply congested, but nowhere consolidated.

Heart—Healthy; firmly contracted.

Abdomen—Spleen slightly enlarged; very soft. Kidneys—Left kidney was very large and congested; ureter healthy. Right kidney was exceedingly small, so as to be scarcely recognised at first as such; the parenchymatous portion was smaller than the pelvis, which was distended with pale limpid fluid; only a few of the pyramids remained, and the glandular substance was nowhere one-fourth of an inch thick from pelvis to capsule. Right ureter was very much thickened, its walls greatly dilated, and appeared to be about the thickness of the little finger. Bladder apparently healthy. Liver abnormally adherent to the diaphragm, and a scar with a kind of puckered contraction was discovered on the diaphragmatic surface of the right lobe; on section it was very dark in coloured, and the vessels obviously very much congested.

Microscopic Examination of Tumour—The peripheral portion of the tumour consisted of small-celled structure, but its internal portion was structureless. It was surrounded by thick connective tissue, in which could be seen numerous small, thickened blood-vessels. The specimen under the microscope showed the disappearance of the nerve fibres from the cortex of the brain in the immediate neighbourhood of the tumour. The latter was probably of syphilitic character.

LUPUS OF LARYNX.

By WALTER G. SMITH, M.D., F.R.C.P.;
Physician to Sir Patrick Dun's Hospital.

[Read in the Section of Pathology, March 21, 1890.]

A YOUNG man who had been affected with lupus of the nose and face, of many years' standing, was operated upon by Dr. Bennett, about ten years ago, and supplied with an artificial nose.

The laryngoscope showed that the larynx was also involved. After some years chest symptoms developed, and in the middle of February, 1890, he was suddenly seized with acute pain in the left side, and severe dyspnœa set in. The physical signs of pneumothorax were fully established, and he died on the following day.

Post-mortem Examination.—Right side of the chest flattened; left side bulging. Upon opening the abdomen the liver was found to be uniformly enlarged, extending 3½ inches below the umbilicus, and it weighed 7½ lbs. The arch of the diaphragm reached to the fifth interspace on the right side, while on the left side it had descended to the eighth interspace, displacing the stomach and spleen. When the thorax was opened a quantity of gas escaped from the left side, and the lower lobe of the left lung was completely collapsed. The heart was flattened and pushed over towards the right. There was no fluid in either pleura, and the right lung was firmly bound down upon all sides by thick, old-standing adhesions. Numerous vomicæ, containing thick, yellow pus, existed in each lung, and tubercle bacilli were found in the lung. The spleen, liver, kidneys, and intestines gave the reaction of amyloid degeneration.

Larynx.—Base of epiglottis thickened; free edge irregularly eroded, and, to a great extent, destroyed. On the posterior surface were two firm conoidal projections, one above, and one extending below the vocal cords, which had nearly disappeared.

FRACTURE OF THE STERNUM AND COSTAL CARTILAGES, WITH RUPTURE OF THE HEART.

By E. H. BENNETT, M.D., M.Ch., F.R.C.S.;
Professor of Surgery in the University of Dublin, &c.

[Read in the Section of Pathology, March 21, 1890.]

I am indebted to Drs. M'Craith and M'Carthy, former pupils of the School of Physic, for the opportunity of presenting these rare specimens to the Royal Academy. The man from whose body they were taken was aged fifty, and was a wagon examiner at one of our railway stations. On a dark winter's evening last December he was going to his work about 6 30 p.m.; he was found on the line dead, without anyone being aware how he was killed, except that it was evident that he had been struck by a passing engine.

There were no external marks of injury on the body, except that the scalp was torn from behind forward all but off, being attached only over the brows. There was a fracture of the left temporal region of the skull, and there had been bleeding from the ear, but there was no fracture of the base of the skull. One thigh was broken, and there was an injury of one elbow. There was probably a fracture of the pelvis, but the hasty performance of the *post-mortem* examination prevented the detailed investigation of these injuries. The abdomen was not opened, as the coroner was satisfied with the mortal injuries disclosed before this stage of the investigation was reached, and ordered that it should be left unopened. The chest showed no external sign of injury, but when the soft parts were divided down the mesian line in front it was discovered that the greater part of the sternum, with parts of the costal cartilages attached to it, was entirely detached from the ribs. Only the upper part of the manubrium, and its connections with

the first pairs of ribs and the clavicles, were intact. The lower part of the manubrium, with small pieces of the second pair of costal cartilages, the gladiolus, with parts of the remaining cartilages of the true ribs, and the ensiform cartilage, were quite separated from the chest-wall.

It is of interest to examine the faces of the fractures of the costal cartilages, for they are all smooth and broken across so sheer that a hasty observer might readily mistake their appearance for that of sections made by a knife. The constancy in the costal cartilages of this fracture *en rave* is well known, but I have never before seen so many or such perfect examples of it in one subject.

The pericardium was found to be full of blood, and the heart has suffered the following remarkable injuries:—

The anterior surface of the infundibulum of the right ventricle is indented, and the pericardial covering and the muscular structure are torn for the extent of an inch directly across the surface. On opening the chamber it is seen that a more extensive laceration of the endocardium and of the muscular wall of the infundibulum has occurred. This rent is V-shaped; one limb of the V, which inverted, starts from the site of the anterior transverse wound, and runs up to the interval between the anterior and left valves of the pulmonary artery. From this point the second limb of the V passes down along the posterior wall of the infundibulum to terminate opposite its fellow. One of the muscular pillars at the springing of the infundibulum is partly torn through. Passing to the posterior surface of the heart to the parts directly behind the injuries I have described, we find that the left auricle and its appendix are extensively torn in the space that exists between the entrance of the left pulmonary veins and the groove separating the auricle and ventricle. Three great rents in the wall of the cavity traverse this region in parallel lines, so that the entire tissue is in shreds. I have taken these two series of injuries first in my description, as I think it is evident that they are the result of a single force, the crushing of the heart by the upper extremity of the broken sternum driven backwards against the spine. The shape of the wound on the anterior surface of the right ventricle corresponds with the sharp

fractured margin of the bone which itself fits the depression made in the wall of the cavity.

I pass now to the second series of injuries which the heart has suffered. The apex of the right ventricle and the adjoining thin edge of the heart shows a great rupture with ragged, everted edges; the opening is an inch and a quarter long on the surface of the chamber. The right auricle presents a large rupture along the auriculo-ventricular groove large enough to admit a finger freely, and the right auricular appendix two small openings. The lining membrane of the posterior wall, and the muscular tissue of the auricle are torn along a line which leads into the opening of the inferior vena cava, and here the pericardial surface is also torn. Looking at this second series of openings in the heart, I think the explanation is to be found by assuming that the crush of the heart against the spine, which caused the first series of ruptures, by direct pressure, produced these latter by making the blood burst through the walls of the right ventricle and auricle. The left ventricle alone has escaped injury.

NOTE ON DIPHTHERITIC MICRO-ORGANISMS.

By EDMOND J. McWEENEY, M.A., M.B.;
Pathologist, Mater Misericordiæ Hospital; Lecturer on Pathology, Catholic University Medical School.

[Read in the Section of Pathology, May 2, 1890.]

The specimens which, with a few explanatory remarks, I have the honour of laying before the Academy to-night, consist of (*a*) sections through the epiglottis of a child which died of pseudo-diphtheritic laryngitis in the Mater Misericordiæ Hospital, and (*b*) cover-glass preparations of a pure culture of the *Klebs-Löffler bacillus*, the organism to which diphtheria is now ascertained to be due. The patient whose epiglottis I sectioned had been admitted in the desquamation stage of scarlatina, with a bad throat and nephritis. On *post-mortem* examination, I found the mucous membrane of the upper part of the larynx coated with a thin layer of greenish exudation. The sections stained by Weigert's modification of Gram's method, show the nuclei coloured red, and in addition numerous violet patches, which, on examination with the homogeneous immersion objective, prove to consist of micro-organisms. Some are cocci, some bacilli. They are scattered mostly throughout the structureless necrosed part of the mucosa. They are probably only saprophytic, and of no pathogenic significance, for the bacilli are smaller than Klebs-Löffler's, and are distinguished in addition by the fact that they stain by Gram's method, whereas the Klebs-Löffler is at once decolorised by the KI. Cornil and Babès also describe organisms found in cases of pseudo-diphtheritic laryngitis after scarlatina, but they seem to have found mainly cocci. In this connection I thought it would be interesting to bring before the Academy, for the sake of contrast, a cover-glass preparation of a pure culture of the genuine diphtheric

bacillus, with which the names of Klebs and Löffler are associated. The specimen shows the strange "involution-forms" very clearly. All doubt as to its pathogenicity has been removed by the researches of Spronck,[a] who found that a sterile filtrate of a pure culture would cause (in animals) a paralysis closely resembling the post-diphtheritic variety in the human subject—likewise albuminuria.

[a] Zur Kenntniss der pathogenen Bedeutung des Klebs-Löfflerschen Diphtherie Bacillus. Von Prof. Dr. Spronck (Utrecht). Centralbl. f. Path. Anat. 1890. No. 7.

CASE OF MULTIPLE SMALL ABSCESSES OF THE LIVER.

By JOSEPH REDMOND, M.D., F.R.C.P.;
Physician to the Mater Misericordiæ Hospital;
Senior Physician to Cork-street Fever Hospital.

[Read in the Section of Pathology, May 2, 1890.]

Mrs. E., married, aged thirty-three, was admitted into hospital on the 27th February, 1890. She had enjoyed good health up to the first of November, 1889, when she contracted typhoid fever. Towards the end of that illness she suffered on one or two occasions from attacks of hepatic colic. She, however, made a good recovery, and was discharged from hospital on the 9th of December, 1889, feeling quite well and strong. Her health continued good for about six weeks, when she again began to suffer from repeated attacks of severe pain over the liver. Not being able to obtain any relief, and finding that her strength was failing, she sought admission to the hospital on the 27th of February, 1890.

On her admission her state was as follows:—The patient was anæmic, with slight flush on the malars, jaundiced, and somewhat wasted in appearance. She lay on her back, and complained of severe pain over the liver, which caused her to place her hand on her side in order to obtain relief. Her stomach was irritable, no food having been retained for some days, the vomited matter yellow and bitter to the taste. The bowels were constipated, and the last motions observed were somewhat light in colour. Her tongue was furred; temperature, 98°; pulse, 100; respiration 24. The abdomen was tender over the liver, the hepatic dulness somewhat increased. The spleen was enlarged, and could be felt below the ribs. Scattered râles were audible over the bases of both lungs. Heart healthy. Her condition remained unaltered for a few days, the symptoms not undergoing any marked change till the 5th of March, when she had a severe and prolonged rigor. On the following days there was a recurrence of the rigor, and on the 8th of March, the pain, which was complained of on the right side,

extended to the left, and was accompanied by some difficulty of breathing, signs of pleuritis being detected over both bases. Her symptoms continued somewhat the same till the morning of the 12th of March, when the temperature fell to 97°; pulse, 132; respiration, 24. The temperature, pulse, and respiration being respectively 99°, 180, 52, a few hours before death.

The autopsy was made by Dr. M'Weeney, pathologist to the hospital.

The following are the notes of the examination :—

Thorax.—Lungs—Left pleural cavity contained about ten ounces of serum, with flakes of lymph, both floating freely and attached to the lung itself; left lung partly consolidated at base. Right pleura also acutely inflamed; about 20 ounces serum with numerous flakes; general appearance yellowish milky. The right lung itself had a small abscess at the apex the size of a pea, containing very thin pus, and its visceral pleura was coated with greenish lymph. Pericardium inflamed, flakes of lymph and a few ounces of fluid present. Heart normal.

Abdomen.—About twenty ounces of straw-coloured serum in peritoneum. Spleen enlarged to about fifteen ounces; pulp almost diffluent, no abscess. Liver somewhat enlarged; soft, pale, numerous small abscesses in left lobe, especially posteriorly and superiorly; they varied from size of a pea to that of a marble, seemed to be lined with a distinct membrane, and contained greenish-yellow pus mixed with reddish-yellow flaky material; liver substance around abscess softened and congested. Portal vein contained dark semi-fluid blood. Gall bladder: wall white, greatly thickened, full of small angular calculi, which were yellow exteriorly, with a black nucleus. Cystic duct thickened, patent, but common bile duct contained a calculus the size of a pea, which lay immediately beneath duodenal mucous membrane, and completely blocked the duct. Intestines healthy, no ulcers; matter in duodenum bile-stained. One of the mesenteric glands formed a calcified mass, the size of a marble, of stony hardness. Kidneys large, congested, soft. Pelvic organs healthy.

Remarks by Dr. M' Weeney.—The interest of this case is largely due to the fact that gall-stones here co-existed with a state of things which, at least in the *post-mortem* room, closely resembled pyæmia. The question at once arises, were the abscesses of the liver and lung, the enlargement of the spleen, the pleurisy, and pericarditis due to a general pyæmia arising from some other cause (undiscovered), or were the calculi at the bottom of the mischief? Now, it is at first sight difficult to understand how biliary calculi could cause acute abscess of the liver, though Carrington (in "Guy's Hospital Reports for 1883," p. 387) and Fagge ("Practice of Medicine") seem to look upon this as possible. It seems to me that biliary calculi could only produce such abscesses, the pus of which contained a very great abundance of cocci in chains, as was demonstrated at the Academy of Medicine, indirectly, either—(*a*) by causing ulceration of the bile duct near the duodenum (a gall-stone actually lay in this situation). Organisms from the interior of the gut, finding their way up the bile duct, could establish themselves perhaps on such a solution of continuity of the protective epithelial lining, and make their way back into the liver (the abscesses seemed to lie in or around the larger ramifications of the bile duct)—or (*b*) by raising the pressure in the ducts to such a degree as to lower their vitality, thus making them a *locus minoris resistentiæ* for any organisms which might be circulating in the blood. We know that organisms do circulate sometimes in the blood without being able to ascertain how they obtained admission; the facts of ulcerative endocarditis prove this. Taking all the facts into consideration, the view of the pathology of the case that seems most probable to Dr. Redmond and myself is as follows:—The abscesses in the liver were indirectly due to the biliary calculi, perhaps according to one of the chains of causation mentioned above; general infection of the system followed, indicated by the state of the serous cavities, the abscess in the lung, and the tumefaction of the spleen. Had the patient survived longer a much larger number of abscesses would probably have been formed.

A CASE OF ALVEOLAR SARCOMA OF THE TESTIS.

By EDMOND J. McWEENEY, M.A., M.B.;
Pathologist, Mater Misericordiæ Hospital; Lecturer on Pathology, Catholic University Medical School.

[Read in the Section of Pathology, May 2, 1890.]

I will not detain the Section long over this case, the interest of which is purely histological. The testis which I exhibit this evening was removed from a man, aged thirty, two months ago, at the Mater Misericordiæ Hospital by Mr. Chance.

The patient had first noticed the swelling about two years previously, and it had since increased gradually and painlessly, with absence of testicular sensation. No tubercular or syphilitic history of patient or family, and no history of injury was obtainable. The testis was enlarged to the size of a medium-sized orange; it was globular and hard. On section the substance was white, mottled with yellowish patches, which looked to the naked eye like caseated portions, and which, on microscopic examination, were fatty-degenerated and almost devoid of structure. There was no trace of tubercular new growth. The white tissue consisted of cells and a stroma. The cells were largish, oval, uni-nucleated, and offered little or no variation in size or shape. They did not lie in actual contact, but each was separated from its neighbour by a small quantity of homogeneous intercellular substance. Their characteristics were, on the whole, of the connective tissue rather than of the epithelial type. The stroma was trabecular in character, the main trabeculæ being comparatively thick, and running a straight course through a considerable part of the sections. From them were given off more delicate bands, which in their turn gave origin to still more delicate ones—the same structure prevailing

throughout—namely, round and spindle-shaped nuclei of various sizes, with little or no approach to the formation of fibrous tissue. The ultimate trabeculæ consisted of but one or two rows of spindle cells placed side by side and end to end. They were clearly distinguishable from the oval cellular elements above mentioned, which lay in groups of varying size (about a dozen together as a rule) in the ultimate meshes of the stroma. I am in some doubt as to whether the stroma does not represent that which in the normal testis, starting from the mediastinum, runs between the lobules, in which case it would seem to have increased, *pari passu*, with the tumour; or whether it was of entirely new formation, in which case the specimen would have to be looked upon as one of alveolar sarcoma, a neoplasm of some rarity in that situation. On the whole, I am inclined to take the latter view.

It is not necessary for me to make any further remarks. The microscopic sections which I have displayed under several microscopes, will, I think, bear out my opinion as to the nature of this unusual neoplasm.

FRACTURE OF THE HEAD OF THE RADIUS.

By E. H. BENNETT, M.D., M.Ch., F.R.C.S.;
Professor of Surgery in the University of Dublin, &c.

[Read in the Section of Pathology, May 30, 1890].

This example of fracture of the head of the radius which I desire to submit to the Academy of Medicine is of interest chiefly for its rarity. I regret that I can give no life history of it, as I have obtained it from the body of an elderly woman during the course of operative surgery in the School of Physic this session.

A careful examination of the limb which contained it showed that no other fracture, either of the bones of the forearm or the humerus, was associated with it, nor was there any evidence that dislocation of the bones of the forearm had occurred at any time, for the structure of the several ligaments of the joint showed no trace of such accident.

The line of the fracture as it traverses the cartilage of the head of the bone is clearly marked. It divides the cup-shaped depression which articulates with the capitellum of the humerus into two almost equal parts, and passes down on either margin through the surfaces which articulate with the lesser sigmoid cavity of the ulna and the coronary ligament respectively, and can be indistinctly traced on the corresponding surfaces of the neck of the bone. There is no projection caused by callus on any part of the line, either in cartilage or bone, but the fracture has been firmly united by bone.

The line of fracture in the cartilage of the radial cup passes almost diametrically across it in a line parallel to the anterior surface of the shaft. The anterior half was evidently detached from the rest of the bone and displaced slightly beneath its fellow.

At first sight the position of the line of fracture, and the direction of the displacement of the anterior fragment, would suggest that the injury was the result of undue pressure breaking off the

fragment, as the radius tended to be displaced directly backwards, as in cases of dislocation of both bones backwards at the elbow, under conditions in which the fracture has been found in association with fracture of the coronoid process of the ulna. The circumstances I have mentioned bear against this view.

If the bones of the forearm are fully pronated, and so viewed in relation to the articular surfaces of the humerus placed in relation with them, it is seen that when the joint is almost fully extended while the radius is pronated, the line of fracture marks off the surface of the unbroken cup, which is free of the capitellum posteriorly, from the half detached by facture, which, in this position, is in contact with the capitellum. It is very unlikely that a shock acting in the line of the axis of the radius would occur while the hand was held supine, while the prone position of the wrist with an extended elbow is more likely to be that in which such shock would occur.

This specimen is exceptional, like two or three of those already recorded, in being uncomplicated by fractures of the coronoid process of the ulna, complete or incomplete, or by fractures of the radius elsewhere, or of the humerus. In the absence of any life history it is useless to speculate further on the cause, except, perhaps, to note that there is here an entire want of evidence of any great injury such as has been commonly noted in the few recorded examples of the fracture; there is no fracture of any other bone of the body, nor anything to suggest that any great injury had caused the fracture.

Although completely united, and devoid of any traces of recent injury, the specimen shows one other point worthy of note. On the surface of the capitellum a trace of the degeneration of chronic rheumatic arthritis is present, not in its idiopathic form, such as one might expect to meet in an old woman, the inhabitant of one our workhouses, but a recently-established disease which has not yet worn through the cartilage of the capitellum, and which, not being present in the opposite joint, points to its being the result of the injury. Adopting this view, the accident must have occurred not more than a year or so before death.

FINAL NOTE ON A CASE OF LEPROSY.

By JOSEPH O'CARROLL, M.D., F.R.C.P.;
Physician to the Richmond, Whitworth, and Hardwicke Hospitals.

[Read in the Section of Pathology, May 30, 1890.]

In February, '89, I exhibited to the Academy a case of leprosy The history of the patient up to that date will be found in the Transactions, Vol. vii., p. 296.

The patient continued under my care, and was confined to a detached room and a small space of garden near it. He remained in practically the same condition as when exhibited, until November '89, when he began to suffer from iritis in the left eye. As a consequence of this and the caries of the feet, which had become more troublesome, he was unable to leave his bed during the greater part of the winter. Towards the end of March, '90, he suffered from paroxysms of dyspnœa, which gradually increased in severity, while a soft systolic murmur, best heard at the lower end of right border of the sternum, was detected. He died of cardiac failure on the 9th May, 1890.

I examined his body seventeen hours after death. It was well nourished; there was much subcutaneous fat, and a large accumulation of fat existed between the rectus abdominis and the peritoneum. Some superficial ulcers were found on the fingers of the right hand and one on the ulnar margin of the left. Scabs closed the sinuses in the feet. I examined the right foot, and found a small mass of granular *débris* around the head of the second metatarsal bone. The corresponding phalanges had been removed about two years previously.

Slight pleural adhesions existed, especially at the upper limits of the cavities. In the apex of each lung I found one or two encapsuled caseous masses, the largest of which, about 5 mm. in diameter, was partially cretified. The heart was enlarged, much

covered with fat, and with some fatty change in its muscular tissue. The tricuspid orifice was dilated, the wall of the right ventricle thinned, and the whole right heart was filled by a large firm decolorised clot. A single calcified atheromatous patch, about 12 mm. in diameter, and covered by the intact inner coat of the vessel, was found in the aorta near the innominate orifice.

The tip of the epiglottis was ulcerated, the vocal cords were thick and rigid, and the laryngeal and pharyngeal mucous membrane was everywhere thickened. The palate was much ulcerated. The œsophagus was normal in appearance. The stomach showed patchy congestion, especially at its cardiac end; at the pylorus were two little wart-like papillæ. The liver showed slight increase of interstitial fibrous tissue, but was otherwise normal. The adrenal bodies were unusually firm. The kidneys were normal except for slight fibrous hyperplasia. The spleen was normal.

The brain, cord, and meninges were normal. I examined the main nerves of the upper and lower limbs. The right ulnar, both internal popliteals, and the right external popliteal were thickened in lengths varying from one to three inches. In the left ulnar just above the elbow joint was a blue-black oval tumour 1½ inch long by something more than ½ inch across, which proved to be an extravasation of blood within the nerve-sheath. The other nerves examined seemed normal.

A thick fibrous envelope enclosed epididymis and testis in one oval mass, about once and a-half the size of the normal organ.

I examined certain of the tissues by the Ziehl-Neelsen process as modified by Günther (see Clinical Diagnosis, by Von Jaksch, translated by Cagney. 1890. Page 77). I examined a large number of sections of each tissue.

I failed to find bacilli either of leprosy or tubercle in the lung nodules, toe-joint *débris*, stomach, pylorus, spleen, adrenals, and in some of the nerve tissue.

In addition to the tissues in which I had previously discovered the bacilli lepræ, as detailed in my previous communication, I now found the bacilli in large numbers in the enlarged nerves and in the testis.

In the enlarged nerves there was considerable overgrowth of fibrous tissue, which seemed to encroach upon many nerve strands. The bacilli were mostly grouped in masses. I think I have detected bacilli within the neurilemma, but I cannot be sure.

The sections of testis show the bacilli in large number, and in larger masses than any other tissue. Some of these masses are three or four times larger than those elsewhere, and would, therefore, have perhaps ten times the diameter of an average leucocyte. I have not, however, made accurate measurements of them. The condition of the testicle throws doubt on the conclusion expressed in my previous paper that the enlargement there noted was originally traumatic.

I am inclined to consider, contrary to the opinion expressed, I believe, by Unna, that the masses in which the bacilli lie are really cells. For although their usual shape is more or less spherical, yet I have seen them as long sharp-ended ovals in longitudinal sections of nerves, and of the typical bipolar shape in connective tissue. Further, in sections from which the magenta stain has been largely removed, I have seen in some of the masses a pale spot without bacilli which was strongly suggestive of a nucleus. In other sections from which the magenta has been still further withdrawn the spot has taken the blue colour in advance of the adjacent substance. These appearances were most marked in longitudinal sections of nerves.

THE BENIGN NEOPLASMS OF THE RECTUM.

By CHARLES B. BALL, M.Ch., F.R.C.S.;
Surgeon, Sir Patrick Dun's Hospital, and University Examiner in Surgery.

Read in the Section of Pathology, February 14, 1890.

Morbid growths are met with in the rectum and at the anus with great frequency, and, as compared with other portions of the intestinal tract, the fact that this part of the gut can alone be examined during life with facility, renders the proportion met with in this region apparently large, yet it cannot be denied that the termination of the bowel is very much more frequently affected than the other portions by these growths. This is more particularly noticeable in the case of piles, or hæmorrhoids, which constitute by far the commonest tumours of the rectum and anus, but are unknown in the other portions of the intestinal tract. Owing to the more potent sources of irritation to which the lower bowel is subject, the greater liability to tumour formation can readily be accounted for, and in the case of hæmorrhoidal tumours there is a distinct anatomical reason for their existence here. During the passage of a solid mass of fæces along the great intestine, the contraction of the muscular tissue causes a considerable pressure to be exercised on the blood-vessels. In the upper portions of the intestinal tube this is rather salutary than otherwise, as the vessels here are for the most part arranged at right angles to the axis of the bowel, so that the contraction of the tube empties the veins into the larger channels, and thus materially aids in the circulation of blood; but when the termination of the rectum is approached the conditions are materially altered, because the blood from the lower portion of the bowel passes up by the superior hæmorrhoidal veins, the tributaries of which originate at the junction of the skin and mucous membrane. These vessels instead of being arranged at

right angles to the axis of the intestine, as is the case higher up, are parallel to the axis, lying immediately under the mucous membrane, so that as the fæcal mass is pressed onward by the contracting muscular wall, there is a tendency to force back the column of blood in the veins towards the tributaries, which are therefore liable to become dilated and varicose; indeed, a slight amount of varicosity of these veins is held by some authors to be normal. This liability is further augmented by the fact that the radicles of the superior hæmorrhoidal veins have but trivial anastomosis with the inferior and middle hæmorrhoidal veins, so that but little relief can be afforded to the distended veins by the passage of blood *via* the inferior and middle hæmorrhoidal veins into the general or caval circulation.

Various terms have been used to designate the forms of hæmorrhoids, external and internal, the former of which are subdivided into venous, cutaneous, and compound, while in the latter the venous, columnar, and nævoid varieties are recognised. Now, although these varieties possess points which render them clinically important, it cannot be maintained that they are distinguished by marked pathological characteristics. When taken broadly, the pathology of all is practically identical—namely, increase and dilatation of the blood-vessels, more particularly the veins with proliferation of the connective tissue. In the case of external piles the covering is the scaly epithelium of the anal canal, while internal piles are covered with mucous membrane.

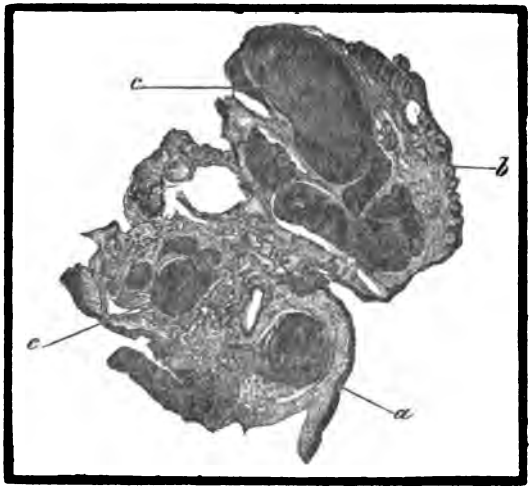
I have elsewhere endeavoured to show[a] that extravasation of blood is not an important factor in the pathology of piles. In the simple venous external pile which appears apparently suddenly, what really happens is this: A varicose condition of one or more of the inferior hæmorrhoidal veins has previously existed without attracting the attention of the patient; it is only when thrombosis and perivascular inflammation supervene that the tumour is noticed, and the patient has what is known as "an attack of piles." That this is indeed an intravascular, and not extravascular, coagulation of blood can easily be demonstrated. If an incision be made

[a] The Rectum and Anus, their Diseases and Treatment.

into a simple inflamed external pile a blood-clot can be turned out, leaving a smoothly lined cavity, in which an endothelial lining can be observed. Moreover, as a result of stretching of the anus—now the prelude to almost all rectal operations—extravasation of blood is of very common occurrence, yet we invariably see it as a widely diffused ecchymosis, which is rapidly absorbed, and not as a localised blood-clot similar to what we know as an external pile.

That, except in external covering, there is no essential difference in the structure of an internal and an external pile, the following specimen will show. This and the other illustrations of this communication are from the microphotographs kindly made for me by Professor Scott.

Fig. 1.



Section of intero external pile × 3 diameters.

a. Skin of anal canal. *b.* Mucous membrane.

c. Laminated blood clots in the interior of dilated veins.

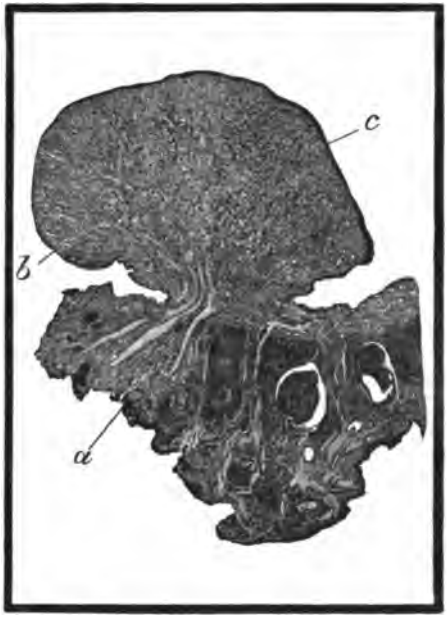
Here it may be seen that above and below the mucocutaneous juncture large thrombosed veins (*c*) are filled with laminated coagula, and between these there is considerable proliferation of the connective tissue in which moderately large arteries ramify. The mucous membrane (*b*) covering the internal portion has lost

much of its character from frequent prolapse, but is still quite recognisable.

External piles which have been frequently thrombosed eventually become converted into but little else than connective tissue covered with scaly epithelium, the large dilated veins disappearing, the whole structure being not much more vascular than the surrounding skin. This is called the cutaneous or fleshy pile. A precisely similar change may take place in internal piles, resulting in the production of what is known as fibrous polypus of the rectum. This change, however, is not so common as in the case of external piles, no doubt owing to the more frequent irritation to which the latter are liable. From an examination of a somewhat large series of pile sections, it can be demonstrated that in cases of tolerably long-standing internal hæmorrhoids, the characters of the section illustrated in Fig. 1, which was taken from a case of recent origin, become somewhat modified; the dilated and thrombosed veins are less remarkable, and the connective tissue more abundant and fully developed. This is a change which can be readily recognised in the living body. The soft and brighter red internal hæmorrhoid becomes firmer to the touch and whiter in colour—indeed, in extreme cases, the covering is almost the same colour as the adjoining skin, instead of the bright red colour of normal mucous membrane. Once this change has occurred, the altered structure tends to become more and more pedunculated until the fibrous polypus, attached by a more or less narrow pedicle, is produced.

The specimen from which Fig. 2 was taken was removed from a case of twenty years' standing. The lower extremity of the rectum was completely surrounded with typical pile structure from which were hanging five firm fibromata, attached by narrow pedicles. These I believe to have originated in frequently prolapsed piles, a theory which I have been enabled to support by tolerably numerous observations on the intermediate stages of growth taken from a number of cases.

Fig. 2.

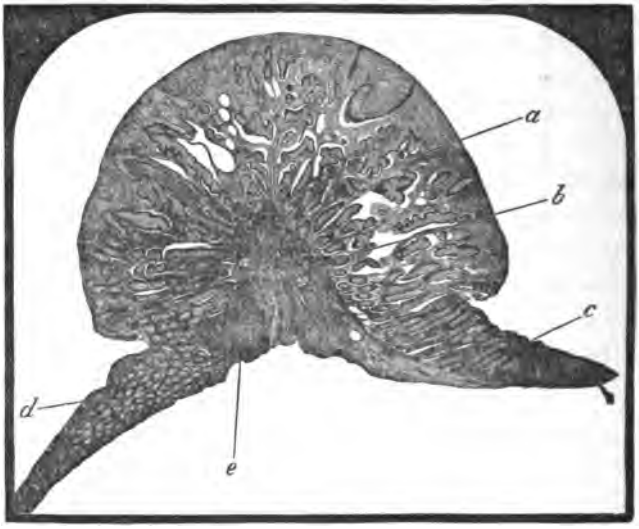


Fibrous polypus of rectum × 2½ diameters.
a. Hæmorrhoidal structure. *b.* Fibrous tissue.
c. Altered mucous membrane.

Adenoma.—The polypus met with in early life is usually a true adenoma, and although probably occurring in all parts of the intestinal tract, is met with most frequently in the rectum. This may probably be due to the more frequent irritation to which this portion of the bowel is subject. It commences as a proliferation and general enlargement of the Lieberkühn follicles, which causes the mucous membrane to buckle up. When this has reached a sufficient size to offer an obstruction to the passage of fæces, and be dragged upon by peristaltic movement of the intestine, a pedicle is formed. When the tumour originates high up this pedicle becomes gradually elongated until the polypus touches the anus, and is finally extruded at defæcation; it may then be strangulated, or the pedicle ruptured and the tumour voided; while in other cases

prolapse or intussusception may be induced. These tumours rarely grow to a large size, and are usually single. They may, however, occasionally be multiple, and in a few rare cases the entire surface of the mucous membrane may be studded with them.

Fig. 3.



Adenoma of rectum × 10 diameters.

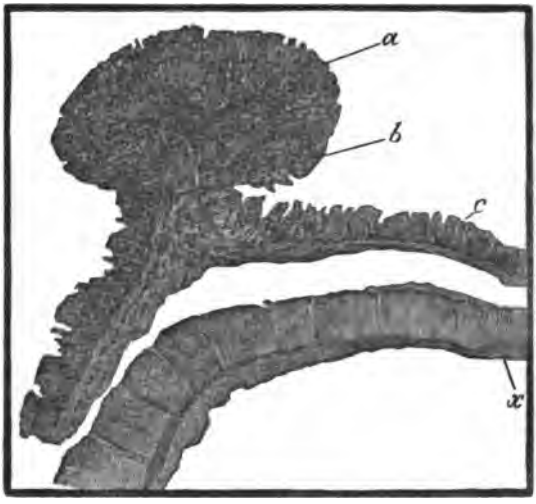
a. Glandular structure.
b. Hilum of connective tissue and vessels.
c. Mucous membrane cut longitudinally.
d. Mucous membrane cut transversely.
e. Solitary gland.

Fig. 3 was taken from a child who came into hospital with a prolapse, at the apex of which two small polypi, the size of peas, were found, but as yet no distinct pedicles had formed. The section was made through the hilum of the largest of the growths, and shows the proliferation of gland tissue with the central mass of connective tissue, from which septa radiate; the section has traversed a solitary gland.

The adenoid tissue found in purely benign polypus differs structurally in no respect from that found in the most malignant of rectal cancers, the great majority of which are of the adenoid

type, except that in the former the tendency is to project into the lumen of the bowel, and become pedunculated; while in the latter the muscularis mucosæ is rapidly perforated, the muscular tissue of the bowel infiltrated, while the ulceration, invasion of surrounding structures, implication of lymphatic glands, and occurrence of metatastic growths, which characterise carcinoma in other parts of the body, become manifest. In some cases of cancer, however, pedunculated growths are met with, but they are unlikely to be confounded with the simple glandular polypus, as the induration of the surrounding structures, the irregularity of the growth, and the discharge, will suffice to make the diagnosis. The way in which this occurs is that as the cancerous infiltration is destroyed by ulceration, islands of tolerably healthy mucous membrane are left, and these tend to become more pedunculated as the ulceration spreads. We do, however, meet with apparently benign adenomatous growths, coexisting with malignant disease.

Fig. 4.



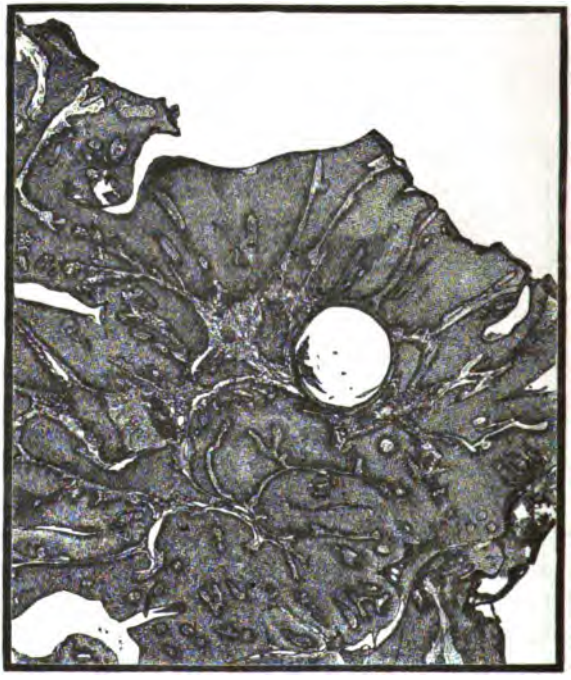
Adenoma of rectum from case of rectal cancer × 12 diameters.

a. Glandular structure. *b*. Connective tissue and muscularis mucosæ.
c. Healthy mucous membrane. *x*. Muscular coat of bowel.

The section from which Fig. 4 was taken was kindly given me by Dr. Patteson; it was taken from a case in which undoubted rectal cancer was present, with little pedunculated growths of unquestionably benign character, as indicated by the non-involvment of the muscular coats of the bowel; these were situated at some distance from the cancerous ulcer, and were probably caused by the irritation of the discharge from the cancerous ulcer passing over the mucous membrane. I have on more than one occasion seen similar little polypi on the prolapsed mucous membrane of colotomy wounds in cases of rectal cancer.

Papillomata of the anus differs in no respect from similar structures in other parts of the body when covered by scaly epithelium.

Fig. 5.



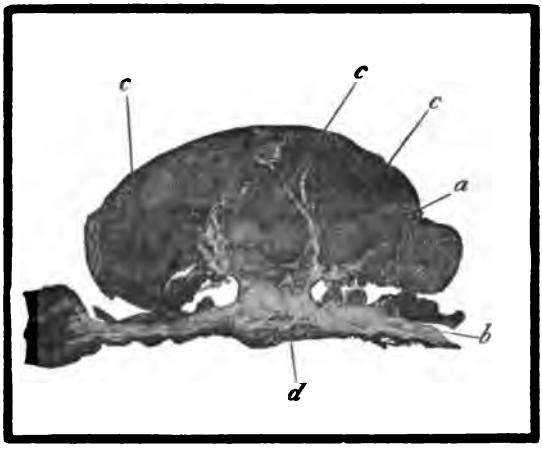
Papilloma of anus × 15 diameters.

Fig. 5 was taken from a typical case, and illustrates very well

the structure; they sometimes grow to a considerable size. I have removed one of a quarter of a pound in weight. When occurring in the interior of the rectum they are known as villous tumours, but as yet I have been unable to make a minute examination of a well-marked case of this variety.

Myomata are rarely met with in the rectum; they form firm more or less pedunculated growths, originating in the muscular tunic, and having the mucous membrane freely movable over them. I have some sections kindly placed at my disposal by Dr. Macan from a case in which he enucleated a tumour of this nature from the wall of the rectum; it resembles closely the similar tumours so commonly met with in the uterine wall.

Fig. 6.



Lymphoma of rectum × 5 diameters.

a. Remains of mucous membrane. *b.* Muscular coat of bowel.
c. Solitary glands much enlarged. *d.* Pedicle.

Lymphoma.—Fig. 6 represents a section of a polypus, from a boy aged six, the subject of a prolapse of the rectum, at the apex of which two small growths, attached by short pedicles, were seen and removed. The general character resembled that of ordinary adenomata, but, upon minute examination, were found to consist almost entirely of lymphoid tissue. Presumably they originated in the solitary glands of the mucous membrane. In the section

illustrated apparently three of the solitary glands (*e*) have grown together and greatly increased in size; they appear to have pushed their way through the mucous membrane, small remnants of which may still be seen at several points (*a*) in the section.

Infiltrating lymphosarcoma has been observed in several portions of the intestinal tract, but pedunculated growths—like the one illustrated—unassociated with obvious lymphoid disease in other parts of the body, is, as far as I know, previously unobserved.

Amongst the pathological curiosities which have been found in the rectum we find records of lipomata, teratomata, enchondromata (?), and diffused angioma, which, I believe, complete the list of benign tumours of this region.

SECTION OF STATE MEDICINE.

ON THE MEDICAL SELECTION OF LIVES FOR ASSURANCE.

By ARTHUR WYNNE FOOT, M.D., Univ. Dubl.;
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[Read in the Section of State Medicine, February 7, 1890.]

LIFE and fire are the objects in view of which assurances are now mostly effected. No longer are they, as formerly, negotiated against thieves, highwaymen, death from drinking Geneva, loss of female virtue, divorces, or making boards out of sawdust.

The early offices, it is true, charged enormous premiums owing to their not possessing reliable data for calculation; and perhaps they were not unreasonable in doing so, for some of their undertakings, if we may judge from the enumeration just given, were extremely hazardous, and involved risks not always avoidable.

The contract of life assurance is one based on a full knowledge by the insuring company of the *health* and *habits* of the person insured. Upon the selection of lives depends to a large extent the financial success of a Life Assurance Company. The duties of the medical examiner—on whom very largely depends this selection—are responsible, and in some respects peculiarly so. There may be no difficulty in at once coming to the conclusion that certain lives are eligible, and that others are quite ineligible; but there is a large number of intermediate lives that will try the skill and tax the patience of the examiner, demand mature consideration, and involve further inquiries, before they can be finally disposed of.

It is now universally admitted by all parties interested that the medical element constitutes an important item in the consideration

of a proposal for life insurance. The medical examiner's relation to the proposer is different from that which he occupies in his ordinary professional capacity. In the latter case the patient exposes his infirmities, and even intensifies them; the applicant for life assurance, on the other hand, may desire to lessen their importance, or conceal their existence. In fact the applicant for assurance stands in a relation the very reverse of that occupied by a patient. The latter comes for relief, and is ready to aid, as far as he can, in the discovery of his ailment. The former approaches with the assumption of health, and the examiner must detect unaided any fallacies in that assumption; and it may so happen that the would-be assurer is a rogue, and his object and interest are to deceive.

The questions set forth for answer in the Proposal Form are sometimes regarded by medical examiners, not permanent officials of a company, as superfluous and vexatious. They are irritated by what seems the dictation to them of a special method of examination, and resent the inquiries made by an office as an imputation upon their mode of reporting. They do not remember that the answers to these inquiries are preserved for reference, and that legal decisions have turned on their having or not having been asked and answered. These questions are also useful in suggesting subjects that a careless or over-busy practitioner might slur over or forget, and no matter how acute and vigilant a medical adviser may be, many points may elude his inquiry or observation, owing to the necessary limits of time and space, as well as to the reticence, wilful or accidental, which many exercise on matters of health. It may be laid down as a general rule that the best men give the most satisfactory and detailed reports, as they fully appreciate the aid they afford towards the development of a social scheme of the highest value and importance.[a] It has long been settled law in these countries that any untrue answer to the questions devised by an assurance company to elicit the knowledge requisite for their contract vitiates the policy, and discharges the company from their liability under it. So far from regarding the present list of ques-

[a] Sieveking. The Medical Adviser in Life Assurance. 1874. P. 48.

tions as vexatiously minute, I anticipate that before long assurance companies will not be satisfied without information as to the condition of the retina; the state of arterial tension; the integrity of the various reflexes (superficial, deep, cranial, organic); the nutrition of nerve-fibres and muscles, as estimated by electricity; the centesimal excretion of urea; microscopical examination of urinary sediments; and a statement of the amount of respiratory capacity.

The regular employment of medical men to examine candidates for life assurance is a practice of comparatively recent date. The custom of the old offices, prior to about 1820, was for each life proposing to sign a very distinct and binding declaration to the effect that the individual had not had "cow or small-pox, or any other disease tending to shorten life." No mention seems to have been made of family history. The proposer had to give a reference to "two persons of good repute, one, if possible, of the medical profession." Persons who did not appear before the directors, or who could not refer to a medical gentleman, were required to give a reference to three persons.[a] The disastrous consequences of dispensing with a medical examination was shown by the late Dr. Brinton, who examined a number of insurances effected without medical examination. From three or four pages of these cases he collected no less than forty instances of death from pulmonary consumption, at periods which averaged eighteen weeks from the date of effecting the policy, but often did not exceed three, four, or five weeks. The average loss to the company on these forty policies was exactly forty times the premiums paid.[b] It is in the mortality from tubercular diseases—the very class which, under the old system, was the bane of assurance companies—that selection shows the most powerful influence in favour of the companies. Mr. Dovey,[c] of the Standard Life Assurance Company, London, in a paper read before the Institute of Actuaries, shows that under the modern system, in the first year of assurance, the actual deaths

[a] Sieveking. Loc. cit. P. 43.
[b] Brinton. Med. Select. of Lives for Assurance. 1856. 2nd ed.
[c] Assurance Magazine. Vol. XXIII., p. 285. 1881.

from these diseases are less than the tenth of the computed number; in the second year the proportion increases to 27 per cent.; in the third, to 52 per cent. of the computed mortality. After the third year, the proportion, although showing fluctuations, appears to settle down to an average of 60 per cent.—this ratio not showing any tendency to increase in the latter years of assurance. Thus, after the "immediate effects of the medical examination have passed away, there remains a more permanent, though less strongly marked, influence in the selection of the mortality for a long series of years." This beneficial result may be due to different causes; to the class selection of assured lives; to their increasing age; to the number of predisposing causes which can be appreciated; and to the increased accuracy of skilled personal examination.

In reference to the list of questions asked on the Proposal Forms, there are some points which may be adverted to with advantage to the more inexperienced examiners. The conservatism with which insurance offices cling to the recognition of an apoplectic habit is singular, as it is well known there is no type of face or form characteristic of the conditions which lead to this catastrophe. The apoplectic habit, in as far as it is characterised by the conjunction of broad chest and shoulders and short neck, with a large abdomen, powerful muscular system, and a reddish face, is found absent in the majority of those who suffer from cerebral hæmorrhage, and who are more often poorly nourished, lean, scraggy, long-necked persons. Nothnagel is inclined to think that the popular fear of apoplexy in one who may be short, stout, thick, and red, is based upon a confusion of this affection with cerebral hyperæmia and its attendant symptoms. However, it is a very old belief that there is a particular conformation of body in those who are prone to attacks of apoplexy, the pattern being denoted by a large head and red face, short thick neck, and a stout, square, squat build of body. This opinion dates from the time of Hippocrates; its correctness has become doubtful to modern pathologists. Dr. Wilks repudiates it entirely. It is probable that apoplexy—that is, cerebral hæmorrhage in consequence of disease of intracranial arteries—may befal any who are endowed with the ante-

tecedent arterial conditions, irrespective of their particular build of body. A more important fact to bear in mind is the undoubtedly hereditary character of apoplexy; "it runs in families"—*i.e.*, in an indirect way, in virtue of the inheritance of a tendency to arterial degeneration. The influence of heredity may induce a seizure at an earlier age than 40, before which age apoplexy otherwise rarely occurs. In 116 claims for apoplexy against the Scottish Widows' Fund, the youngest victim had reached the age of only 22. His father had died of the same disease at 66.

There is seldom any attention directed in the Proposal Forms to the existence of a discharge from the external ear, yet such a condition betokens incalculable dangers to the life of the individual who has such a symptom. The importance of attention to aural discharges, in regard of prognosis, was long ago insisted on in this city by Sir William Wilde, and it is not put too forcibly by Dr. Patterson Cassells,[a] when he says that, for general guidance, it may be accepted as an axiom that, with an ear discharge—whether moderate or copious in quantity, continuous or interrupted in its flow, short or long in its duration—*so long as it exists*, one cannot tell how, when, or where the case may terminate. He suggests that the question should be put to proposers, "Have you at any time suffered from an ear discharge?"

Inquiries are seldom made as to whether the proposer is liable to attacks of erysipelas. Erysipelas is a disease of diminished vitality, and its occurrence—especially if more than once—should be regarded with suspicion, particularly in those whose occupation exposes them to intemperate habits, or to sharp, cold winds. I was acquainted with an old clergyman who lived at Kingstown; he had seven annual attacks in succession of facial erysipelas, contracted periodically during the spring easterly winds. He died in the eighth attack with head symptoms. In the case of the Scottish Widows' Fund, Dr. Begbie found erysipelas had proved fatal in eight cases, and that it was associated, as a cause of death, particularly with disease of the brain and its membranes, and disorders of the liver and bowels. Considering the frequency of this associa-

[a] B. M. Jour. Vol. I., p. 323. 1877.

tion, the disposition of the disease to recur, and its connection with depraved digestion and defective assimilation, Dr. Begbie was of opinion that those who have been affected with it cannot be considered as eligible subjects of life assurance.[a]

Another matter which should not be overlooked is the occurrence of fæcal obstruction, especially if originating near the right iliac fossa. The applicant may deem it unworthy of mention, regarding it as constipation; but many people are liable to a recurrence of obstruction, probably from some malformation or band of adhesion, and succumb eventually in the absence of the timely assistance which had rescued them on previous occasions.

The exact state of health at the time of insurance does not represent the whole of the risk incurred by an office. The restoration of health after an attack of pneumonia may be only temporary; it may be speedily followed by phthisis. A convalescent from rheumatic fever may hasten to get his life insured before the signs of valvular disease originating therefrom have become objective. A subject of winter cough may get himself accepted in midsummer, during his æstival intermission, and die of bronchitis early in the ensuing winter. A patient with angina pectoris might easily pass in an interval between the paroxysms. Some practitioners entertain the opinion that, provided they can certify that the applicant is in good health at or about the time of the insurance, this is all the insurers need know. The same opinion is commonly held by the candidate, and a proposer, after having been attended by one medical man for an illness, will apply to another, a comparative stranger, to certify as to his condition of health for insurance. On this account the directors of a company require the applicant to furnish them with the names, if any, of his different medical attendants. Policies have been vitiated by the insured saying they had not had any medical attendant when they had, and verdicts given for the offices on the ground of fraud.[b] In the case of Maynard against the Pelican Life Office, Lord Tenterden remarked that to refer to one medical man whom the

[a] Assur. Mag. Vol. VIII., p. 336.
[b] Tidy. Legal Med. Part I., p. 397.

insured knew would speak well of his health, and not to others whom he knew could not report favourablly (although the insured did not die of the disease for which they had attended him), would vitiate his policy. Similarly, for the insured to refer to a practitioner who had attended him three years previously, and to omit to mention one who had attended him more recently, or to refer the office to a practitioner who had attended him for many years for mere trivial complaints, and to omit one who had attended him, even though for a short period only, for a serious disease, would equally render the policy void.[a]

It may here be worth remarking that, when a medical man is appealed to by an assurance office respecting the health of one of his patients desiring to insure—leaving the company out of the question—justice to the proposer, to his survivors, and to the medical man himself, demands that he should report without a shadow of concealment. Unless he is prepared to do this, he had far better decline to report altogether. Further, he is not justified in reporting as the private medical adviser of the person whose life is in question, unless he has the authority of his patient to do so.

The conscientious discharge, in this respect, of his duty by an examiner may lead to the interruption of life-long friendships. William Cullen and Sir Robert Christison, two of the brightest lights of the Edinburgh school, and indeed of the medical world of that time, were on such terms of intimate friendship as to be called *les deux inséparables.* Cullen had a bad attack of apoplexy in Paris, so bad that Dr. Alison, who happened to be passing through Paris at that time, thought he could not recover. He, however, apparently made a good recovery, but about two years after his attack of apoplexy, one day when on duty in the hospital he fell in a fit of epilepsy, in presence of a crowd of students. Every well-informed physician knows that epilepsy after apoplexy usually makes short work with its victim—in two years commonly, or in three years. Cullen determined to marry, although he had had several subsequent fits at moderate intervals. Sir Robert

[a] Tidy. Op. cit. P. 373.

Christison, in his Autobiography,[b] describes what happened:—"On the occasion of his marriage our friendship underwent a severe trial, and did not withstand it. He (Cullen) had to insure his life for the benefit of his wife and family—and heavily for a young man at the outset of life. He referred to me, as his medical adviser and friend, for a certificate of eligibility. I certified that I knew nothing to detract from the value of his life, except his notorious attack of apoplexy and his fits, 'of the nature of which I had not any personal knowledge, never having seen him in one.' He expressed great surprise and disappointment, pressed me very hard to alter the terms of my certificate, said gloomily that I had a much worse opinion of his case than he had ever dreamt of, and at length worked himself up to urge upon me that my opinion would put an end to his marriage. But as I was satisfied that a few years would end his life, I stood firm under this cruel trial; and we parted, coldly on his part—and cold he remained ever afterwards. I had the satisfaction, however, of learning immediately that I was not only right, but likewise his best friend, for his principal trustee, who was also his agent, called on me on the subject, got my unreserved opinion, and, unlike Cullen himself, expressed great obligation to me for my firmness and frankness, because he had in vain tried to get at the bottom of *the fits* by referring even to those medical men who had witnessed them. 'But now,' added he, 'the insurance directors are warned, I shall tell them they must take every means for satisfying themselves as to the nature of these fits; but that there must be no calling of the insurance in question hereafter on the ground of *information being withheld.*' The result was that the physician of the insurance company—a pompous, conceited, superficial man—himself certified the fits as being of little or no moment, and the proposal was accepted for a merely nominal and trifling extra premium. Cullen had the folly to twit me with his success; and the marriage—which never had been endangered, as his agent assured me, by this difficulty—was carried through. But in about two years more the company had to pay £5,000 to the marriage trustees. Cullen died

[b] 2 Vols. 1885. Vol. I., p. 128.

in a paroxysm; and Edinburgh medicine lost one of her most gifted and promising sons."

Consumption is, perhaps, of all the diseases from which selected lives are known to suffer, the one to which most attention has been directed in medical statistics of life assurance, and a result of the increased vigilance of directors and their officers in avoiding consumptive risks is a very much lessened mortality from this cause. Consumption as a cause of death in the family record should never be overlooked by the examiner. If it happens that brothers and sisters of the applicant have died in youth from lesions of the brain or abdominal organs, tuberculosis may be suspected as the cause of death, because among children tubercle generally attacks the brain and the cavity of the abdomen before it appears in the lungs. When the infant mortality in the family history has been great (generally entered in the proposal as died "young," "in infancy," "in childhood," without any further particulars), and the surviving members, including the applicant, are quite young, or he is the only survivor, the risk is very hazardous. It is a sound principle of action in life assurance to recommend the rejection of a life when so many as two immediate relatives are admitted to have died of consumption. The mortality from phthisis is far more due to its undoubted and constant hereditary transmission than to any peculiarities of climate. Parties proposing for assurance, aware how much an unfavourable family history tells against a life, try at times to cover the existence of phthisis by describing it under some other name. Thus a relative is said to have died of "asthma" or "bronchitis," when, on inquiry, the disease is found to have been undoubtedly consumption; and the very convenient expression, "died at" or "soon after childbirth" is constantly used, where the death has been from some constitutional malady—"dying in childbirth," proving on inquiry to mean after some weeks, and "soon after" to mean several months subsequent to that event. A knowledge of how rapid is the progress consumptive disease often makes after delivery gives an air of reasonableness to the death being attributed to the processes of childbirth. The Registrar-General's office presents a means of verifying the cause of death which has

not hitherto being systematically turned to account in inquiries connected with life assurance. As with consumption, so, when there is a strain of insanity in the stock, the examiner must be on the watch for outcroppings of the flaw or fault, speciously veiled in the family history under such doubtful terms as "nervous depression," "nervous debility," "hypochondriasis," "cerebral congestion," or even a liability to "frequent headaches." When a proposer, descended from an insane parent or parents, or with one or two of his immediate relatives so affected, has a family history spotted with such neurotic blots, it will be advisable to decline the life.[a]

The medical examiner should be on the *qui vive* in respect of any suggestion as to the necessity of examining the rectum. Many cases have occurred in which insurance companies have lost money, after a short period of insurance, by the neglect of this method of examination.[b] Dr. Edward Hamilton, in his "Clinical Lectures on Diseases of the Lower Bowel," mentions a case recorded by Dr. Allingham of a gentleman who was about to be insured at ordinary rates, owing to his healthy aspect and the normal state of all his organs, when the history of some bowel trouble led to an examination of the rectum, in which cancer was found to exist beyond question.[c] Cancer of the rectum does not show the same tendency to hereditary transmission insisted on for cancer in other situations, and the symptoms may also be so completely suppressed that it may for a long time elude the notice of the patient himself, his friends, and his medical attendant. Bearing on the latency of cancer of the rectum, there is an instructive case recorded by Hilton Fagge.[d] An old woman died in Guy's Hospital, and he discovered three tumours in the brain, or in its membranes, from whose appearance (columnar epitheliomata) he felt convinced that they must be secondary to a primary growth elsewhere. But he searched every part of the body in vain, until at length he

[a] Assurance Magazine. Vol. VIII., p. 334.
[b] See Med. Press and Circ., 12th June, 1889. P. 629.
[c] Dubl. Jour. Med. Science. Vol. LXXVI., p. 69.
[d] Pract. Med. Vol. I., p. 103.

happened to notice that the extreme lower end of the rectum had not been taken out with the rest of the intestine. This, when removed, was found to contain a large ulcer, with prominent fungating edges, which was evidently the lesion he had been looking for, although it had given rise to no symptoms during life. Even the administration of enemata had failed to lead to its discovery. One may well exclaim with Ovid, "*Felix! quem faciunt aliena pericula cautum!*"

The previous acceptance of a candidate in another or in the same company must not tempt the examiner to relax his vigilance. In the lapse of time since a previous examination the personal habits of the applicant, the development of some hereditary tendency or constitutional disease, or the occurrence of some severe illness or accident may have changed the conditions of the risk, and have affected the eligibility for assurance. As in a *bonâ fide* consultation on a private patient, the fresh adviser generally examines for himself, and forms his own opinion on the case, so ought the examiner of a company treat a candidate who may have been accepted by another company.

In regard to "habits," a young examiner is apt to be satisfied with the stereotyped answer "strictly temperate." But there is no phrase as to the meaning of which opinions differ more than that of "strict temperance." The proposer's idea of what is strict temperance may differ from that adopted by the company, and an answer, truthful from his point of view, may appear the opposite from theirs. The fault generally lies in the wording of the question. The law requires material facts to be disclosed, and questions asked to be truly answered; but if the questions seek information not about facts, but about matters of opinion, it seems fair to throw on those who seek to show the answers were untrue the onus of proving the answer did not agree with the opinion of those who made it. The medical examiner should, by a series of questions, put with professional tact and with seeming indifference of manner, elicit information as to the quantity and nature of the spirituous liquor imbibed, whether he is in the habit of drinking before meals, or upon an empty stomach, and whether or not his

occupation peculiarly exposes him to temptation. A good mode of inquiry is—"What is your usual beverage—beer, wine, or spirits—and their general amounts? Do you take any spirits in the forenoon? Is that a habit?" To prevent trouble, it is worth while to inquire specifically what the proposer drinks, how much, and when. It is difficult to define in words what constitutes "intemperance," and this difficulty opens a door to much discussion and difference of opinion. Thus, in the case of Southcomb *v.* Merriman,[a] the medical man admitted that the insured had had several outbreaks of intemperance, which he did not state, because he thought they had no influence on the health of the applicant. There was a verdict for the plaintiffs (executors of the insured), but a rule for a new trial was obtained; yet twelve witnesses proved the applicant to have been very temperate, and twenty-one that he was the reverse. For insurance purposes the true question is, not what constitutes intemperance generally, but, is there reason to believe that the applicant takes more alcohol than his constitution will bear? In insurance cases the physician must consider the word "intemperance" as a habit prejudicial to the life of the special individual, and not in any broad and general sense. If one man drinks a glass of wine daily, and it makes him intoxicated, he is intemperate if he continue to drink that one glass of wine. But if another man drinks six glasses daily, and they have no bad effect on him, but rather the reverse, he is not an intemperate man even if he continue to drink six glasses daily.

The effects of intemperance exhibit themselves chiefly at a comparatively early age, the maximal rate of mortality in such lives occurring between 41 and 50, so that a person who at that age gives evidence of habitual temperance has escaped one of the most prevalent causes of structural degeneration. Viewed in their relation to acute disease, the behaviour of intemperate people under pneumonia is suggestive of their unfitness to be profitable lives for an insurance company. The Report of the Collective Investigation Committee on Pneumonia, although only provisional, brings out the fact that the deaths from croupous pneumonia among total

[a] Tidy. Part I., p. 399.

abstainers were 11·12 per cent.; among temperate persons, 18·4 per cent.; and among the intemperate, 40·5 per cent.[a] An explanation of this high death-rate which is offered is the inability of intemperate persons to sustain the prolonged calorification of this disease—a view which is borne out by Col. Sykes' observation[b] that, where one teetotaller is cut off in India, four intemperate men lose their lives. The susceptibility of drinking men to insolation is well known, and I would just note in passing that, in the medical history, an attack of delirium tremens is not unfrequently entered as "a touch of sunstroke." *Verbum satis sapienti.*

There are other habits besides those of drinking which prejudice the value of a life, though perhaps not to an equal extent—for example, the use of opium, or tobacco, or even vegetarianism. These matters should be ascertained and duly weighed by a sagacious examiner. His doing so will, at least, prevent objections on the part of a captious company, and obviate possible discussion at a future time. It it not to be expected that the applicant will reveal them unsolicited. Bearing on this point is the dictum of Lord Mansfield—"The insured (proposer) need not mention what the insurer (examiner) ought to know, what he takes upon himself the knowledge of, or what he waives being informed of. The insurer (examiner) need not be told general topics of speculation."[c] And Lord Denman held that a proposed insurer (applicant) was not bound to volunteer statements of all sorts of things; but he was bound to conceal nothing when asked, and to answer all questions truly.[d]

In reference to urinary examination, the change which has taken place in cases of life assurance is little less than a revolution. Brinton, writing in 1856, says, with respect to the urinary apparatus—"Careful inquiry will often give us so healthy a history of its function as to allow us to dispense with all further examination, not only of the organs themselves, but even of their secretion.

[a] Brit. Med. Jour., Dec. 1st, 1883. P. 1080.
[b] Assur. Mag. Vol. VIII., p. 331.
[c] Taylor. Med. Jurisprud. P. 1156.
[d] Tidy. Op. cit. Part I., p. 399.

It is only where gout, scarlatina, dropsy, or some other disease or appearance of a like suspicious character specially directs our research towards these structures that we need alarm or annoy our subject of examination by insisting on any further enquiry." About the same time (1857) Dr. George Johnson[a] was appointed medical adviser of the Equitable Life Office, an appointment which he held for a period of eighteen years. Soon after his appointment he wished to arrange for testing the urine of every applicant for insurance; but the directors objected that this would cause annoyance and give offence, and they begged that he would test the urine only when he had reason to suspect that something was wrong. He had to submit to their decision, but he told them that the result would be the acceptance of an uncertain number of lives with latent albuminuria.

Later on, Dr., afterwards Sir, E. Sieveking,[b] in 1874, contends that as no medical man reports on a case for life insurance without examining the thorax, so no report for life insurance ought to be considered complete without a definite statement as to the condition of the urine after examination by the physician. At the present day the leading offices, so far from regarding examination of the urine as a proceeding calculated to offend or annoy their clients, take good care to see that their rules on the subject are invariably complied with, even in ordinary cases; and I have had the papers returned to me, and have had to have a second interview with the proposer, owing to my not having tested the urine for sugar and albumin in a case in which it proved to be, as I had anticipated, perfectly normal.

Owing to the increasing frequency of systematic and accurate investigations of the urine the knowledge has been acquired that albumin is often found when there is nothing in the state of health to lead to a suspicion of its existence. The subject of such albuminuria may be completely ignorant that anything is wrong with him. For example, a gentleman whose albuminous urine was rendered nearly solid by nitric acid spoke to Dr. George Johnson

[a] Brit. Med. Jour. Vol. II., p. 419. 1889.
[b] Medical Adviser in Life Assurance. P. 152.

in substance as follows:—"I have come to you at the urgent request of my medical attendant; but I am perfectly well, and I want neither advice nor medicine."[a]

For many years past the fact that albumin may be abundantly present at one period of the twenty-four hours and entirely absent at another has been publicly demonstrated, and ought to be generally known. This recurrence of albumin occurs day after day in exactly the same sequence. At the period of rising in the morning there is no albumin to be discovered. In an hour or two's time albumin shows itself, increasing in quantity for a while, and then, as the day advances, declines, and becomes usually entirely lost before bedtime is reached. Remaining absent during the night, it returns after rising the next day, and subsequently follows the same order that had been previously observed. Such cases are called "cyclic" by Dr. Pavy. And this condition may run on with its repeated diurnal variation for weeks, months, or years without anything further becoming noticeable.[b]

To ascertain that a case of albuminuria belongs to the cyclic genus it is manifestly necessary to test the urine, not only after rest in bed and before breakfast, but also during the day and after food and exercise.

There is another group of cases of albuminuria in apparently healthy persons, in which a notable quantity of albumin exists, and is always present, when no history of nephritis can be elicited, no casts of tubules discovered, and none of the cardio-vascular symptoms of Bright's disease recognised. Dr. Pavy says that he would look favourably upon such a case only after it had been under distinct observation for a considerable time without anything wrong becoming developed, and even then he would consider that a decidedly more than ordinary risk existed.[c]

It would only be very inexperienced examiners who would fall into the mistake of thinking lightly of "a mere trace of albumin." The granular kidney is the form of renal disease especially asso-

[a] Brit. Med. Jour. Vol. II., p. 419. 1889.
[b] Brit. Med. Jour. Vol. II., p. 418. 1889.
[c] Brit. Med. Jour. Vol. II., p. 418. 1889.

ciated with small amounts of albumin, and their attention should have been attracted to the coincident conditions of this affection—namely, increased quantity of urine, low specific gravity, and morbid changes in the cardio-vascular system. The granular kidney is constantly shadowed by death from uræmia, from cerebral hæmorrhage, or from inflammation of the serous membranes.

These cases of latent albuminuria—a term, I think, not open to the objections which affect the terms temporary, functional, or physiological—stand in quite different lights prognostically, according as they are regarded as insured or uninsured cases. An albuminuric who has been accepted by a company, and who feels and believes himself to be quite well, and knows he has made provision of some kind for the future, is not as likely to take care of himself as an uninsured, whose rejection has perhaps led to his knowledge of a screw loose where he never suspected it. Frequent medical advice will probably be sought by the latter in trivial ailments, for which the former would scorn it. Dr. Johnson[a] has expressed a decided opinion that temporary albuminuria, even when traceable to food, or over-exertion, or exposure to cold, will, if neglected, sooner or later lead to persistent albuminuria, and to fatal disease of the kidneys. Thus, I do not think the prognostic rules for carefully watched albuminuria will apply to those who are careless and indifferent about an affection which does not trouble them, and hence I would fear that an optimism as to the results of latent albuminuria derived from the results of well-attended patients in private practice might lead an examiner to take a too lenient view of albuminuria when the question of life insurance is involved. The private practitioner or the consultant can afford to make a more favourable prognosis of albuminuria in apparently healthy persons if they are to have the continual superintendence of obedient and intelligent patients; but the medical examiner of an insurance company who passes an albuminuric life is rather in the position of a station master who lets a train move on with the red lamps against him. Albuminuria is always serious, though it may not always indicate organic disease, and in this

[a] Brit. Med. Jour. Dec. 1879. P. 928.

respect it has a very apt resemblance to hæmoptysis. The presence of albumin in the urine, unless when "extra-renal," is always pathological, whether we can detect the nature of the pathological change or not. Albumin present in the urine even casually must be regarded as a danger signal.

It is quite true, leaving insurance business aside, that some albuminurics may live a long time, even outlive people with perfectly normal urine, providing they comply with such conditions as (1) no history of Bright's disease in a parent or in any two near relatives; (2) no personal history of past acute nephritis; (3) no history of plumbism or gout; (4) no cardiac hypertrophy, hard pulse, frequent nocturnal micturition, or retinal changes; (5) where the age is not over 40; (6) where the urine does not deposit casts; (7) where there is no chronic dyspepsia; but such lives belong to the class called risky, and the acceptance of risky lives is too speculative a business to formulate general rules for. It is a significant fact that Mr. Eales, of Birmingham, found retinal changes in five out of fourteen cases of supposed temporary albuminuria in persons between the ages of eleven and twenty-eight.[a]

The only insurance company that is known to have inquired into the subsequent state of health of persons whose lives had been declined on account of albuminuria is the United States Company in New York.[b] Among those who made applications to that office in the three years, 1878–80, there were sixty-nine (or from 10 to 12 per cent. in each year) whose urine was found to be albuminous. Before the end of 1880 four of these persons died, and it is stated by Mr. Munn, one of the medical officers of the company, that the "general appearance of the majority of the others who had been under observation for more than a year was gradually deteriorating." The results of this investigation show that an insurance company runs a great risk if it neglects to have the urine of all applicants tested.

At a meeting of the Association of American Physicians, held in Washington in 1888, Dr. Tyson, of Philadelphia, a well-known

[a] Birmingham Medical Review. 1880.
[b] Hilton Fagge. Pract. of Med. Vol. II., p. 1601.

writer on renal diseases, formulated the conditions under which he considered albuminuria might be regarded as not constituting a formal ground for exclusion in life assurance. These conditions are—1. That the applicant presents in all other respects the signs of good health. 2. That the albuminuria is unaccompanied by tube casts. 3. That the quantity of albumin does not exceed one-fifth of the bulk of the specimen of urine examined. 4. That the albumin is absent on rising in the morning (this is not essential). 5. That the density of the total urine for twenty-four hours is not under 1015. 6. That signs of cardiac hypertrophy or high arterial tension are absent. 7. That the applicant is not over forty years of age. 8. That gout in any shape is not present. 9. That retinal changes are absent. But he qualifies all this by the statement that there are few medical examiners competent to make such an examination as these conditions would imply. Whether this is so or not, I think that for this generation, at all events, the traditional notion should hold good that the presence of albumin in the urine, unless clearly extra-renal, should be regarded, without further question, as excluding the case from acceptance by life insurance companies.

In examining the urine it is well to bear in mind that it should in all cases be tested for sugar regardless of the specific gravity. It is only by routine examination of the urine that the early stage of diabetes mellitus can be discovered, when the quantity of glucose is insufficient to raise the specific gravity much, and it is to be borne in mind that glycosuria, or the transient occurrence of sugar in the urine, may at any time pass into confirmed diabetes. If there appears to be any indication of sugar in the urine, the family history should be closely scrutinised, for diabetes comes next to rheumatism in frequency of heredity. Diabetics are ineligible, because, although some cases live long, the ultimate result is always disastrous. In fact, a diabetic patient may be aptly compared to a tower undermined—its downfall is assured, but none can certainly tell when the catastrophe will occur. The quantity of urine obtainable is often insufficient to float the urinometer freely. In such a case the urine may be diluted with distilled water, and the

last figure of the specific gravity of the mixed fluids multiplied by their united volumes. Thus, if we add to half an ounce of urine one and a half ounces of distilled water, and the specific gravity of this mixture is 1,007, then 7 multiplied by 4 gives the specific gravity of the unmixed urine as 1028.[a]

It might seem to many that the discussion of such a subject as the acceptance of cases of damaged heart is preposterous and irrelevant, but the reading of Sir A. Clark's paper at Brighton (1886) on "Cases of Valvular Disease of the Heart, known to have existed for over Five Years without causing Serious Symptoms,"[b] has raised the question in a very practical manner. This remarkable essay was based on notes of 634 of his private patients by Sir Andrew Clark, who put his note-books into the retort of criticism, and has given us the benefit of the distillate. The result of the analysis goes to show that there exist multitudes of persons with chronic valvular disease of the heart, who not only suffer no inconvenience therefrom, but are also capable of discharging the duties and enjoying the pleasures of life.

The first of his cases may be quoted at length as a type of the class referred to. In the early part of 1842 the house-governor of one of the largest London hospitals being about to marry, petitioned the committee for rooms wherein to dwell with his wife, and proceeded to insure his life. The office refused to accept the insurance upon any terms, and in reply to the urgent questions of the candidate, who averred that in the whole course of his life he had never been ill, said that he had a serious disease of the heart in the form of mitral regurgitation. To the further question of the proposer whether he would live six months, the reply was that he might. Then the house-governor, with the characteristic inaccuracy of patients, informed the committee that he had a mortal disease of the heart, that he could not live six months, and that he withdrew his application for rooms. The committee considered the matter, and fearing that the house-governor might die suddenly in the wards, they superannuated him; and furthermore,

[a] Balfe. Dis. of the Kidneys. P. 53.
[b] Brit. Med. Jour. Vol. I., p. 260. 1887.

believing that he had but a short time to live, they superannuated him on full pay. In 1854 this gentleman consulted Sir A. Clark for indigestion, who discovered that he had a loud, rasping, systolic murmur, heard not only in the mitral area, but all over the left side of the chest. Beyond the symptoms of indigestion, he exhibited no other evidence than the murmur of the existence of cardiac disease. Without being particularly careful, he continued to live, work, and enjoy life until 1874, when, at an advanced age, he died of an attack of acute bronchitis. Thus, for thirty-two years after having been condemned to die of heart disease, he continued to enjoy more than average health, to discharge the duties of a town rectory, and to draw his full pay from the hospital.

The conditions of a favourable prognosis differ for different valves, and for each valve according to the character of the lesion. A comparatively small "loading" might justify assurance in a favourable case of mitral regurgitant disease, whilst no "loading," however heavy, for a time however short, would warrant acceptance of a case of regurgitant disease of the aortic valves. If in the former case the risks to be incurred were to be reckoned as one, then in the latter case the risks to be incurred would have to be reckoned as a thousand. The person with aortic might possibly live as long as the person with mitral disease, but there would be such small security for the transaction that, considering the possibilities of disaster, it could not be considered as other than a reckless speculation.[a]

The conditions which, according to the experience and judgment of Sir A. Clark, would justify a case of mitral regurgitant disease in sustaining an application for life assurance, are as follow, assuming on the part of the patient obedience to properly-adjusted rules of health:—(*a*) Good general health; (*b*) just habits of living; (*c*) no exceptional liability to rheumatic or to catarrhal affection; (*d*) origin of the valvular lesion independently of degeneration; (*e*) existence of the valvular lesion without change for over three years; (*f*) sound ventricles of moderate frequency and general regularity in action; (*g*) sound arteries, with a normal amount of

[a] Brit. Med. Jour. Vol. I., p. 371. 1887.

blood and tension in the smaller vessels; (*h*) free course of blood through the cervical veins; (*i*) freedom from pulmonary, hepatic, and renal congestion.

There are several objections which at once present themselves to the acceptance of damaged hearts, although we probably each know that a creaking pump is capable of many years of moderate wear and tear. Sir A. Clark himself admits[a] that he has nothing to do with any life assurance office, though he frequently has submitted to him cases for arbitration. As in the cases of latent albuminuria, the comfortable or wealthy classes, who are independent of a prospective provision for their families, are in a different category as to prognosis from those who, damaged as they are, having passed a medical investigation, believe themselves sound, knowing and feeling nothing to the contrary. These latter eat and drink, and do things they would not do if they knew they were walking along the brink of a precipice. Looked at from the point of view of the insurance company, the thing to be considered is not so much the value of the individual life, as the value of a mass of such lives compared with a mass of lives without this damage, and viewed from this aspect the office that accepts a life with an organic heart-murmur runs a risk at present not calculated for in its rates. Some years ago an office of high repute[b] made an experiment of taking lives with heart-bruit, but otherwise in good health. This office, however, in a few years discontinued the practice, as it was found that the lives most largely insured fell in before their expectancy; and though by reason of the heavy loading, the office might not ultimately be a loser on the total of these lives, yet enough damaged lives did not present themselves to form a separate class which could be calculated for.

Sir A. Clark is perfectly logical in carrying his conclusions to their legitimate outcome; but my contention is that, however applicable these conclusions are to private practice, they are not equally so to insurance practice, inasmuch as in the former case there is at all events a likelihood of continuous oversight of the patient, while in the latter case the supervision is of a casual and

[a] Brit. Med. Jour. Vol. I., p. 382. 1887.
[b] Brit. Med. Jour. Vol. I., p. 539. 1887.

temporary nature, while the acceptors of the risk remain helplessly saddled with it. It is now generally accepted that Laennec, and subsequently the pathological school, greatly exaggerated the unfavourable prognostic value of heart-murmurs. We must always be careful not to over-estimate the importance of the murmur as a fact, nor to pay too much attention to the mere existence of the sound and too little to the circumstances in which it occurs. Many a case of chronic valvular disease goes on for a long time, provided compensation—which may be taken to mean accommodation of the muscular power of the heart to adverse circumstances—occurs. But compensation is rather a condition to be hoped for than a reality procurable at will. There are no means by which, in any recent case, its advent can be foretold with certainty. When secondary symptoms postpone their appearance we are willing to assume compensation has taken place, but we can neither guarantee its arrival nor insure its permanence.

The opinion that an applicant with damaged mitral valve can be accepted by an insurance office is probably one rather too far in advance of the present practice of the various offices to be judiciously acted on. As it cannot be denied that even the least amount of valvular disease of the heart *may* prevent a man from living out his expectancy, it is only right that the insurance company should have the benefit of the doubt, and not the applicant. Even supposing, first of all, that the mitral disease shall have existed independently of degeneration in the heart; secondly, that it shall have existed for two years; thirdly, that the ventricles shall be in good order; fourthly, that the arteries shall be sound; fifthly, that there is no persistent basic congestion, or recurrent hepatic stasis; that the patient should not be liable to colds which hang about him; and, finally, that his general health shall be good, within which limitations Sir A. Clark[a] is prepared to say that the mitral disease will not shorten life—the application of this doctrine to life assurance would require very great discrimination, and would be a dangerous one to inculcate among any but the most highly informed and most careful examiners.

[a] Brit. Med. Jour. Vol. I., p. 382. 1887.

THE INFECTIOUS DISEASES (NOTIFICATION) ACT, 1889, AND ITS EXTENSION TO DUBLIN.

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[Read in the Section of State Medicine, April 25, 1890.]

Although the subject of notification of infectious diseases was discussed by the State Medicine Section of the Royal Academy of Medicine in Ireland in 1885,[a] there seems good reason for recurring to it, as owing to the passing of the Notification of Infectious Diseases Act, 1889, the question has entered upon an entirely new phase. On the last occasion, whilst notification was generally approved, the chief discussion was as to who should notify, and what fee should be paid if the burden of notification was laid upon the medical man.

These points have been settled by the Act, and already steps have been taken by the authorities to adopt the Act, and from March 1st, 1890, it was in force, and will soon be in active operation in Dublin.

It may be of use to examine the provisions of the Act in order to form an opinion as to what effect its adoption will have.

Although the present general Act was passed only last year, compulsory notification of infectious disease is no new thing. Fifty-nine Urban Districts in England from time to time adopted local Acts, the credit of priority being, I believe, due to Huddersfield. The 67th section of the Huddersfield Waterworks and Improvement Act, 1876, which received her Majesty's assent on July 13th, 1876, required notification, but only in such cases of dangerous infectious diseases as were deemed to be without proper

[a] Transactions of the Academy of Medicine. 1885. Vol. III. Page 405.

lodging or accommodation for the isolation and treatment of the patient. The occupier or patient had to notify, but if a medical man was in attendance he was bound to fill in and give to the patient or person in charge a printed form, which the patient or person in charge had then to forward to the Sanitary Department. These forms, with postage stamps affixed, were supplied in books free to the medical men practising in the district.

I mention these points somewhat in detail, as it was under this earlier Huddersfield Act that I had two years' experience of compulsory notification. During that time I had frequently to notify, and I never experienced any of the difficulties the opponents on theoretical grounds of notification apprehend.

The effect of the passing of the general Act of 1889 has already been to cause a great extension of the area over which notification is compulsory.

Up to the middle of January the new Act had been adopted in some 700 sanitary districts[a] with a population of about 10,000,000, adding London—in which the adoption of the Act was obligatory—with its population of 3,816,683,[b] and the 59 large towns which previously had local Acts, with their population of 3,828,825,[c] and it will be seen that the system of compulsory notification already applies to upwards of 18,000,000 persons, or more than two-thirds of the total population of England and Wales.

By the new Act the local authority of any urban or rural sanitary district in Ireland may adopt the system by passing a resolution after fourteen clear days' notice, by publishing this resolution by advertisement in the local newspapers, with notice of date when Act will come into operation (such date being not less than one month from appearance of first advertisement), and by sending a copy of the resolution to the Local Government Board for Ireland. The steps have already been taken in Dublin, but the preliminary date named for the Act to come into operation in this city has not yet been fixed.

[a] Brit. Med. Journal. Jan. 25, 1890.
[b] Census. 1881.
[c] Census. 1881.

The important clauses of the Act are:—

"3. (*a*) The head of the family to which such inmate (in this Act referred to as the patient) belongs, and in his default the nearest relatives of the patient present in the building or being in attendance on the patient, and in default of such relatives every person in charge of or in attendance on the patient, and in default of any such person the occupier of the building shall, as soon as he becomes aware that the patient is suffering from an infectious disease to which this Act applies, send notice thereof to the Medical Officer of Health of the district:

"(*b*) Every medical practitioner attending on or called in to visit the patient shall forthwith, on becoming aware that the patient is suffering from an infectious disease to which this Act applies, send to the Medical Officer of Health for the district a certificate stating the name of the patient, the situation of the building, and the infectious disease from which, in the opinion of such medical practitioner, the patient is suffering."

These paragraphs do not apply to hospitals in which persons suffering from an infectious disease are received, or structures belonging to her Majesty the Queen.

For failure to comply with the provisions of the Act medical men are liable to a fine not exceeding forty shillings.

Certificates are to be provided free to the medical men living in a district in which the Act is adopted.

The fee payable to the medical man for notifying a case of infectious disease is 2s. 6d. for a private case, and 1s. for a case occurring in his practice as medical officer of any public body or institution.

The following explanation of the term "infectious disease" is given in the Act. It—

"Means any of the following diseases, namely, small-pox, cholera, diphtheria, membranous croup, erysipelas, the disease known as scarlatina or scarlet fever, and the fevers known by any of the following names—typhus, typhoid, enteric, relapsing, continued, or puerperal."

Other infectious diseases may be added permanently or temporarily by the sanitary authority.

The advantages of notification from a public health point of view are obvious. If an epidemic, say of scarlatina, breaks out, it is very important that the Medical Officer of Health should stamp it out as early as possible. But how is he to know of the existence of the epidemic? The first cases are often comparatively light, and it may be some time before one ends fatally; and yet during this time, whilst the earlier cases are running their entire course and the more serious case is progressing to its fatal termination, it is quite possible that the Medical Officer of Health may have no idea that scarlatina has broken out in his district, and infection may be widely spread before the fact of a death from scarlatina. At present we have compulsory notification, but only of deaths, and consequently time is lost just when it is of most value, and before its appearance is made known the disease may have spread so widely that the Medical Officer of Health is unable to follow its ramifications, and all hope of stamping out the epidemic is at an end.

The new Act simply shifts the compulsory notification to an earlier stage, and instead of waiting for a death the Medical Officer of Health is notified of the commencement of the disease. How much better is his position with this information; he can possibly trace out how the disease entered into his district, he can probably find out how many have already come in contact with the patient, and if the sufferer cannot be isolated in his own home he can have him removed to hospital, his room disinfected, and those in the house prevented from spreading the infection in schools or elsewhere. By these means the outbreak of a threatening epidemic may be averted, and that without injury to the patient or his friends. The patient will be better looked after in hospital than at home, and if he is capable of such sentiments will rejoice that he has not been the cause of inflicting injury and danger on others. The friends will be quickly freed from infection, and will be able to return to school and business much more rapidly than if the case had been kept at home.

Mr. John Simon well summed the matter up in the International Medical Congress, 1881, by quoting the famous receipt, "First

catch your hare, and then skin it,"[a] and observing that for disinfection and isolation to be of effect we must obtain early information of the outbreak, whereabouts and nature of disease, and this can be effected only through an efficient notification of disease. The late E. D. Gray, M.P., "held that infectious disease was very like a fire—if notice of it was not received at once it could not be dealt with."[b]

Could these advantages be gained by voluntary notification? I believe the only answer we can give to this question is—certainly not. Some good may be done, as has been the case in Dublin, where the dispensary medical officers have for some time notified, and where the particulars of cases admitted into the fever hospitals have been sent to the Public Health Department. But such notification can never be general, and if not general there must always be the danger that the unnotified cases will be foci for the dissemination of infection. In addition to this, notification when not compulsory cannot be practised without laying the medical attendant open to the charge of a breach of professional trust, as he is voluntarily sending particulars of his patients' illness to the sanitary authority. When notification is compulsory, this objection is removed.

The Medical Officer of Health of Aberdeen writes[c]:—"The number of cases reported was twice as many when notification was compulsory as when it was voluntary."

The Medical Officer of Health to Leeds (Dr. Spottiswoode Cameron, who was Medical Officer of Health to Huddersfield during my residence there, and to whose professional skill and administrative ability I have good reason to testify) gives some striking figures illustrating his experience at Leeds. He writes[d]:—

"Turning now to the question, what proportion of cases have probably been reported to us at Leeds? I find that during the 52

[a] Quoted by Dr. J. W. Moore. Transactions Social Science Congress. Dublin, 1881. P. 542.

[b] Transactions Social Science Congress. 1881. P. 547.

[c] Has the Dual Notification of Infectious Diseases been a Success? By Robert Farquharson, M.D. Sanitary Record. November, 1889.

[d] Report on Notification and Hospital Accommodation. By Dr. Spottiswoode Cameron. Leeds, 1889.

weeks of the past year, 1889, we have heard, in one way or another, frequently through the death returns, of some 598 cases of scarlet fever, of 21 cases of diphtheria, and of 427 of typhoid or enteric fever. During the same period 111 deaths were registered from scarlet fever, 15 from diphtheria, and 111 from typhoid fever. Supposing the fatal cases to have been 5 per cent. in scarlet fever, and 10 per cent. in typhoid, the numbers heard of should have been not 598 and 427, but 2,220 and 1,110 respectively. But to speak of the 'number of cases heard of' is really misleading, as will be seen when we remember that, in a large proportion of them, the information was only obtained from the registration of the patient's death. A good many other cases were only 'heard of' when the patient was convalescent, and a request made that we should carry out disinfection. The report books have been examined to ascertain how many of the cases 'heard of' were reported during life. Of 598 cases 'heard of,' in 78 the patient was dead when the inspector visited the house. Of diphtheria, in 14 of the 21 cases heard of, the information came after the patient's death, and in 61, out of 427 cases of typhoid fever, the fatal issue had occurred before we could render assistance. The cases, therefore, heard of during life were not 598, but 520 of scarlet fever, or, with a 5 per cent. mortality, only one case in every five; in typhoid fever, instead of 427, 366, or, with a 10 per cent. mortality, we heard of only one case in three, often only after the opportunity of doing service was passed."

From time to time numerous objections have been urged to compulsory notification, generally by those who have had no practical experience of its working. I regret to say that I myself expended two guineas in opposing the notification clause in the Huddersfield Bill. I stayed long enough in the town to regret, not only my guineas, but also my opposition to an Act the benefits of which I soon recognised.

One of the most extraordinary objections to the Act is that it interferes with the liberty of the subject; when discussed at the Social Science Congress, 1881, in Dublin, a delegate from the Vigilance Association for the Defence of Personal Rights came

over from London to oppose it. It is generally considered that when the exercise of one man's liberty interferes with the liberty of others it is degenerating into licence. Even if a man has a personal right to have an infectious disease, we have a personal right to refuse to have it communicated to us, and it is in defence of *this* personal right that the Act is intended.

A more widely urged objection is that notification is a breach of professional confidence. Dr. Cameron[a] answers this objection very well:—"Is it not a breach of professional confidence should he notify? Certainly, if he do so of his own mere motion, and without the authority of the patient or his guardian. So-called 'Voluntary Notification' can only be done by consent. The case is entirely different when the legislature steps in and insists upon notification. There is then no breach of confidence on the part of the practitioner. . . The compulsion shifts the responsibility from his shoulders to the broad ones of the community."

Besides, the medical man can always explain to the patient that the information is of a strictly confidential kind, and will not go beyond the health office, and that the duty of the medical officer is to see for himself, or be satisfied by others, that sufficient isolation is being practised, and due precautions taken against the spread of infection. I would venture to suggest to the Medical Officer of Health that he should supply medical men with leaflets explaining the provisions of the Act, and the compulsion of notifying which rests on the medical man.

Nobody complains of having to notify the cause of death, and yet this is not even kept secret, as anyone can ascertain the entry for a shilling. To certify as to death from scarlatina brings down the health officials and their disinfectants just as surely as notifying the existence of the disease.

Another objection is that the Act will lead to concealment of disease, and prevent the assistance of medical men being sought. There is a great deal of truth in this, but it applies to disinfecting. At present the fear of disinfecting leads to concealment of disease;

[a] Hints as to the Working of the Infectious Disease (Notification) Act, 1889. By J. Spottiswoode Cameron, M.D.

the new Act imposes a penalty, and so will ultimately lessen the concealment. This is the great advantage of dual notification—if a medical man is not called in, the onus of notification still rests upon the person in charge of the patient.

W. H. Michael, Q.C., says[a]—"As to declining to call in medical assistance, it must be remembered that in such a case the duty still remains charged in the occupier to forward on his own account the certificate, and if medical assistance were declined on this ground, and fatal consequences ensued, the person so offending would be liable to a criminal indictment for neglect."

Injury to business is also urged as an objection. This objection is a confession that without notification the business would be carried on, in many cases with the certainty of spreading infection. When a disease is allowed to spread injury to business often results, but early notification, and prompt removal to hospital, with disinfection of a single room, will allow the business to go on as usual. A case heard Sept. 13th, 1889, shows the danger of the present system. A dealer in old clothes, who had positively refused to allow his child to be removed to hospital for scarlatina, was convicted for having allowed the child when peeling to play with other children in the street, and to be about amongst the second-hand clothes in his shop. Scarlatina in milk shops also is not unknown.

A more serious obstacle is the difficulty of recognising obscure cases, and that there is no provision for reporting cases that are doubtful. Dr. Cameron, in Huddersfield, got medical men to mark doubtful cases with a note of interrogation, which got over the difficulty to some extent, but certainly the Act ought to make provision for such cases. To report wrongly or to postpone reporting—each is a danger.

In some places the want of hospital accommodation is a difficulty. This ought not to be a trouble in Dublin, as there are a larger proportion of fever beds to population than in most English towns. Dr. Thorne Thorne estimates that in manufacturing towns one bed per 1,000 of the population would be required. Dr. Cameron[b]

[a] Transactions Social Science Congress, 1881. P. 538.

[b] Dr. Cameron, Report on Notification and Hospital Accommodation. P. 5.

asked the medical officers of 42 towns in which notification is already compulsory, whether they considered Dr. Thorne Thorne's estimate too large—30 replied in the negative, and 9 in the affirmative. Most of those replying in the affirmative were, however, not large manufacturing towns. At present in Dublin we have nearly double this amount, or about one bed for each 500 of the population, but I hope hospitals will be prevented from mixing infectious diseases. This should be considered exposing in an infectious condition under the Public Health Act.

The smallness of the fee is sometimes objected to. From the point of view of a medical man, a guinea fee would be more satisfactory; but, from a ratepayer's point of view, it would not be so pleasant. It must be remembered that the payment of a fee, no matter how small, is a concession. Certificates of death and of vaccination have to be given without any fee at all. There are already heavy charges upon the funds available for sanitary purposes, and I do not think a larger fee could fairly be demanded. At any rate the fee cannot be altered now, as it is fixed by the Act.

It has been said that notification has increased the death-rate. A more extraordinary and unfounded statement has seldom been made. In some towns the death-rate has fallen, in a few it has risen; but notification is too recent, and not sufficiently widespread, to allow of any certain effect being traced on so fluctuating a quantity as the death-rate from infectious diseases, but the apparent tendency is to a decrease. The Medical Officer for Birkenhead writes[a]:—"The extraordinary allegation that compulsory notification in places where it exists has increased the mortality from diseases requiring to be notified, is certainly not borne out by facts in Birkenhead; the mortality from diseases requiring to be notified has, since compulsory notification came into force in the borough, so much decreased that it has been less than half the average of the corresponding mortality of recent years."

Another objection is that medical men will be liable to actions for notifying wrongly. Up to the present this objection is theoretical, no such action having taken place. Judges would probably

[a] Quoted by Prof. Corfield in *The Times*.

require to be convinced of absence of *bona fides*, or of reasonable skill, before allowing a medical man to be punished for such a mistake. At any rate the fear of this possible injury may induce medical students to devote more time to the study of infectious diseases than they generally do at present.

What may be expected from the Act? From my own experience and reading I believe it will have a very good effect. It will at first be difficult to work, and some friction may be expected, but the difficulty will not fall on the notifying medical man, but on the Public Health Department. It will add greatly to the Medical Officer of Health's responsibility, but he possesses plenty of tact, and can be trusted to keep down all unnecessary friction. The real difficulty will be with his subordinates, whether they will prove fit to administer the Act. It will take all the administrative power of the Medical Officer of Health to manage this. It is plain that the sanitary officers should be properly qualified; their work under the Public Health (Ireland) Act is at present responsible, but in future, under the new Act, it will be much more so. Having been a faithful servant for many years, or a faithful political supporter for whom no other post can be found, ought not to be sufficient qualifications. All employed should be compelled to go through a course of study, and pass an examination held by some independent body before being allowed to commence work.

I deal with this point as notification *per se* is valueless, it is the use made of the notification which is of value, and much of the popularity of the Act—at first at all events—will depend upon the manner in which use is made of the notifications, and its success is largely in the hands of those employed to carry it out.

One thing I am sure of is that the Medical Officer of Health can depend upon the loyal co-operation of the medical men of his district, and they will try to popularise the Act by explaining its provisions to their patients, and showing them how important it is for the welfare of the community that notification and isolation should be practised.

It has been said that Medical Officers of Health like the Act because it gives them great power, and that the bulk of the pro-

fession disapprove of it. In Huddersfield we all subscribed to oppose the clause of the Bill, and yet, during the two years I was under its provisions, I never heard a medical man say a word against it, and Dr. Cameron, who, after carrying out the Act for twelve years at Huddersfield, has lately been appointed to Leeds, in writing of the practical difficulties of carrying out the Act, says, "No medical man gives us any trouble;" and again, "The medical profession have loyally carried out its provisions, and have also been always willing to give me what assistance I require and what information I seek."[a]

The Medical Officer of Health for Edinburgh (where notification was introduced in 1879) sums up the advantage as follows:—"The infectious clause has been loyally obeyed by the profession, and no complaints have reached the authorities from the powerful medical school, the press, or the general body of practitioners. Lodging-house keepers and hotel proprietors are thankful to be relieved of responsibility, schools have gratefully acknowledged the benefit of the new arrangement, and the authorities the being able to determine every morning the state of the public health as regards infectious diseases, and to track out satisfactorily the nature of mysterious outbreaks of some of them."[b]

I trust that when the Academy comes to review the working of the Act, our Medical Officer of Health will be able to use similar words.

[a] Hints as to Working of the Infectious Disease (Notification) Act. P. 27.
[b] Quoted by Dr. Farquharson in Sanitary Record. Nov., 1889. See also Transactions Social Science Congress, Nottingham. 1882.

THE STATE MEDICINE QUALIFICATION.

By THOMAS W. GRIMSHAW, M.A., M.D., F.R.C.P.;
Diplomate in State Medicine (Dubl.);
Registrar-General for Ireland.

[Read in the Section of State Medicine, April 25, 1890.]

The question of the conditions under which qualifications in State Medicine should be conferred has recently come prominently before the profession, owing to the action taken by the General Medical Council at their meetings of May and November of last year (1889).

The circumstances under which the General Medical Council have taken action in the matter of qualifications in State Medicine are somewhat peculiar.

Under section 21 of the Medical Act of 1886 power was conferred to register a diploma for proficiency in Sanitary Science, Public Health, or State Medicine, provided it appeared to the Privy Council or to the General Medical Council to deserve recognition.[a]

The General Medical Council possessed these powers for two years before they took any action to exercise them, and it was apparently not until the Local Government Act of 1888 came into force that the Council thought it necessary to do anything in

[a] Medical Act of 1886—49 & 50 Vic., cap. 48.—Sec. 21. Every registered medical practitioner to whom a diploma for proficiency in Sanitary Science, Public Health, or State Medicine has, after special examination, been granted by any college or faculty of physicians or surgeons, or university in the United Kingdom, or by any such bodies acting in combination, shall, if such diploma appears to the Privy Council or to the General Medical Council to deserve recognition in the *Medical Register*, be entitled, on payment of such fee as the General Council may appoint, to have such diploma entered in the said Register, in addition to any other diploma or diplomas in respect of which he is registered.

the matter. The eighteenth section of that Act, although a step in the right direction, appears to me to be extremely weak; it is as follows:—

"18. (1) Except where the Local Government Board, for reasons brought to their notice, may see fit in particular cases specially to allow, no person shall hereafter be appointed the medical officer of health of any county or county district, or combination of county districts, or the deputy of any such officer, unless he be legally qualified for the practice of medicine, surgery, and midwifery.

"(2) No person shall, after the first day of January, one thousand eight hundred and ninety-two, be appointed the medical officer of health of any county, or of any such district or combination of districts, as contained, according to the last published census for the time being, a population of fifty thousand or more inhabitants, unless he is qualified as above mentioned, and also either is registered in the *Medical Register* as the holder of a diploma in sanitary science, public health, or State medicine, under section 21 of the Medical Act of 1886, or has been during three consecutive years preceding the year 1892 a medical officer of a district or combination of districts, with a population, according to the last published census, of not less than twenty thousand, or has, before the passing of this Act, been for not less than three years a medical officer or inspector of the Local Government Board. (Sec. 18, Local Government Act, 1888.)"

It will be observed that unless the county contains 50,000 persons according to the last published census, it is deemed unnecessary to require that the medical officer of health should have any special qualification for the discharge of his duties; so that people who have the misfortune to be members of a community numbering less than 50,000 may have their arrangements for the prevention of disease under the medical charge of a person who has never given any special attention to this branch of his profession.

The only cause I can see for the Medical Council not taking early steps to carry out this part of the duty is the careless and weak way in which Parliament discharged theirs, and the date of 1892 as the commencement of the compulsory provision.

Whatever weakness Parliament may have shown in the enact-

ment of sec. 18, it was quite clear by the introduction of the provision regarding a minimum population of 50,000, and from the preceding sections of the Act, that it is intended that a scientific expert in sanitary science should be appointed to the important administrative post of medical officer of health for a large district, that he should devote his whole time to his duties and should be well paid for his services. That this was the intention of Parliament is pretty well shown by section 17, which immediately precedes, and is evidently intended to act as a prelude to sec. 18, already quoted. Sec. 17 of the Local Government Act, 1888 (51 & 52 Vic., cap. 51), reads as follows:—

"(1) The Council of any county may, if they see fit, appoint and pay a medical officer of health, or medical officers of health, who shall not hold any other appointment or engage in private practice without express written consent of the Council.

"(2) The County Council and any district Council may from time to time make and carry into effect arrangements for rendering the services of such officer or officers regularly available in the district of the district Council, on such terms as to the contribution by the district to the salary of the medical officer, or otherwise, as may be agreed, and the medical officer shall have within such district all the powers and duties of a medical officer appointed by a district Council.

"(3) So long as such an arrangement is in force, the obligation of the district Council under the Public Health Act, 1875, to appoint a medical officer of health shall be deemed to be satisfied without the appointment of a separate medical officer."

Now what have the Medical Council done towards providing such a high-class officer? Here are their own provisions as set out in the resolutions passed by the General Medical Council, on June 1 and November 30, 1889, in regard to diplomas in State Medicine:—

"(*a*) This Council, having regard to the terms of section 18 of the Local Government Act, 1888, and observing that under that section special privilege is to be accorded to the holders of the diplomas granted under section 21 of the Medical Act (1886), and therein described as diplomas in sanitary science, public health, or

State medicine, thinks it essential to declare, with regard to its own future action under section 21 of the Medical Act (1886), that it will not consider diplomas to 'deserve recognition in the *Medical Register*' unless they have been granted under such conditions of education and examination as to ensure (in the judgment of the Council) the possession of a distinctively high proficiency, scientific and practical, in all the branches of study which concern the public health; and that the Council, in forming its judgment on the conditions of education and examination, will expect the following rules to have been observed:—

"(*b*) A period of not less than twelve months shall elapse between the attainment of a first registrable qualification in medicine, surgery, and midwifery, and the examination for a diploma in Sanitary Science, Public Health, or State Medicine.

"(*c*) Every candidate shall have produced evidence of having attended, after obtaining a registrable qualification, during a period of six months, practical instruction in a laboratory approved of by the Body granting the qualification.

"(*d*) Every candidate shall have produced evidence of having for six months practically studied the duties of out-door sanitary work under the medical officer of health of a county or large urban district.

"The Council not having at present any means of knowing how far medical officers of health may be able or willing to undertake the education of pupils, this Resolution will not at present be insisted on.

"(*e*) The examination shall have been conducted by examiners specially qualified, and shall comprise laboratory work as well as written and oral examination.

"(*f*) The Rules as to study shall not apply to—

"(*a*) Medical practitioners registered, or entitled to be registered, on or before January 1, 1890;

"(*β*) Registered medical practitioners who have for a period of three years held the position of medical officer of health to any county, or to any urban district of more than 20,000 inhabitants, or to any entire rural sanitary district.

"The Executive Committee has power, in special cases, to admit exceptions to the rules for the registration of diplomas in sanitary science, and report the same to the General Council."

It appears to me that the Medical Council have dealt with this

subject in a very hurried and ill-considered manner, and seem to have totally disregarded the opinions of the Irish Medical authorities in this matter, as set out in the following Report:—

"REPORT.

"After full consideration, we, the undersigned representatives of the four Medical Authorities in Ireland granting qualifications in State Medicine, deputed to take part in a conference on State Medicine, called together at the invitation of the King and Queen's College of Physicians in Ireland, beg to report as follows:—

"As regards Resolution (*a*) of the General Medical Council, the Conference foresee that in the not distant future there will be a demand for two classes of qualifications in State Medicine. Of these, one will be required by those acting as medical officers of health in small districts—for example, Irish Poor Law medical officers in charge of districts with a population averaging 6,000 or 7,000. The other qualification must ensure 'the possession of a distinctively high proficiency, scientific and practical, in all the branches of study which concern the public health,' and will be sought by those who may be regarded as experts, devoting their whole time to the public health service, and having charge of large districts—for example, the medical officers of health acting under County Councils in England, or superintendent medical officers of health in large urban sanitary districts in Ireland.

"In view of the foregoing, the Conference are of opinion that it will be necessary to have two grades of qualification in State Medicine. One of these, the lower qualification, should be based on a specified syllabus of subjects, and might be undergone at any time after the candidate has passed his qualifying examination in medicine, surgery, and midwifery. To such an examination the Conference think that Rules (*b*), (*c*), and (*d*) of the General Medical Council should not apply.

"In the case of the other, or higher qualification in State Medicine, the Conference approve the said Rules, and are further of opinion that even a fuller curriculum than that laid down in those Rules should be required, so as to provide special instruction in such technical subjects as Law, Engineering, Vital Statistics, Geology and Meteorology, as applied to State Medicine.

"The Conference desire to point out that the recommendations which they have made are in accordance with the views put for-

ward by the State Medicine Committee of the General Medical Council in their Report dated July 2, 1869.

"Should it be deemed desirable to distinguish the higher from the lower qualification by a separate title, the Conference suggest that the term 'Certificate in Public Health' would be a suitable appellation for the lower qualification, while that of 'Diploma in State Medicine' might be reserved for the higher qualification.

"As a matter of detail, the Conference wish to point out that Rule (*d*), as adopted by the General Medical Council, would be practically unworkable in the case of Ireland at present, where no 'county districts' and only a few 'large urban districts' exist.

"The Conference recommend that a similar reply to the letter of the General Medical Council should be sent by each of the Universities and by each of the Royal Colleges taking part in this Conference.

"Samuel Haughton, *University of Dublin.*
Christopher J. Nixon, } *Royal University of Ireland.*
David B. Dúnne, }
John William Moore, *K. & Q.C.P.*
John Kellock Barton, *R.C.S.I.*

"*August* 12, 1889."

I consider this Report very clearly and concisely expresses the defects in the procedure followed by the General Medical Council. The Council appear to have misconceived the extent and nature of the duties required of a medical officer of health of a large district devoting his whole time to the work. Such an officer is, in my opinion, and in the opinion of most persons who have studied the question, essentially an executive and consulting officer, not one for the personal or individual carrying out of preventive measures and sanitary improvements. Two elements in the special curriculum required by the Medical Council point to their holding a contrary view to that which I have propounded; they require certificates of six months' practical instruction in a laboratory and six months' out-door sanitary work. There is not a word about what sort of laboratory the study is to be carried out in further than it is to be "approved of by the body granting the qualification." Is it to be in a chemical, physical, or bacteriological laboratory, or partly in each? If partly in each, the time for study is insignifi-

cant. If not in all, the education will be very incomplete. Many persons believe that the Medical Council thought of nothing but a chemical laboratory, and that the education was to be directed towards making the holder of a State Medicine qualification a public analyst. Now this is precisely one of the things I think the medical officer of health should not be. A good public analyst should be a first-class chemist, and such are seldom to be found anywhere except among those who devote their life to chemistry and make scientific chemistry their profession. The medical officer of health has too many and too various executive duties to perform to give the requisite time to the personal conduct of a chemical laboratory. This may not have been the idea of the Council, but they have not indicated any other. As to the second requirement—that of a certificate of "out-door sanitary work"—I am at a loss to interpret the phrase, but it must refer to either inspection of nuisances or the study of engineering works. I defy any one to find out which. A medical officer of health certainly should know how to inspect nuisances, but he should not be an inspector of nuisances; he should also understand how sanitary engineering works are carried out, but it is not his duty to carry them out. What, then, do the Medical Council mean by "out-door sanitary work?" Just like the laboratory work, it is left charmingly indefinite, and the result is that many have interpreted it to mean "inspection of nuisances;" others, taking a higher view of the subject, have interpreted it to mean out-door engineering works—sewage, water, gas, structure of buildings, &c. This certainly is not the interpretation I would put on the phrase—the certificate is to be given by a medical officer of health of a county or large urban district. Far wrong as I think the Medical Council to be in their views about State Medicine qualifications, I can scarcely give them credit for being so foolish as to require a member of the medical profession to certify to engineering knowledge! It may be so, but I can scarcely believe it.

Not only do the Council demand this meagre and indefinite curriculum, but they do not make any specific demand for any instruction in such important subjects as meteorology, engineering,

vital statistics, or law and medical jurisprudence. Why are all these left out? I cannot even conceive an answer to this question. As has been properly implied by the Report of the Committee representing the Irish medical authorities, the Council do not seem to have taken much note of the report of the State Medicine Committee of the Council of July 2nd, 1869. I fancy some have even forgotten the names of the illustrious men who formed that Committee—they were Acland, Christison, Paget, Parkes, Rumsey, A. Smith, Stokes, Thomson. Not one of these men remained to advise the Council, but they had left their opinions behind them, not only in their reports to the Council but also in the personal works of Rumsey—who has been justly styled the father of State Medicine—Acland, and Stokes; and they also furnished the Council with the opinions of nearly all the men in the world whose opinions were worth having at that time. Now, I may say generally that all these authorities concurred in recommending that all the subjects I have just mentioned should be comprised in the curriculum and examinations of candidates for State Medicine diplomas. Have any of these decreased in importance since the year 1869? I should rather think not; they have grown vastly in their proportion, and a great deal more should be expected in 1889 than was thought necessary in 1869. Yet the Medical Council does not think so. A minimum qualification is to be provided to meet the requirements of section 18 of the Local Government Act for a medical officer of health fully qualified in State Medicine and fit to exercise the important functions of a medical officer of health of a district containing a population of 50,000 inhabitants or upwards. All of us interested in the efficiency of public health organisation pressed for the provision now found in section 18. I think Parliament would have taken little notice of our efforts if they had known how little the General Medical Council required to be added to ordinary professional knowledge in order to provide a fully-fledged diplomate in State Medicine.

We in Dublin have a right to feel strongly on this subject. As Rumsey was the father of State Medicine in England, so Stokes was the father and founder of State Medicine diplomas in the

United Kingdom. Dublin was the first University, and the King and Queen's College of Physicians the first medical corporation to provide such a qualification. In a little work entitled "National Health," by Dr. (now Sir Henry) Acland, will be found "the substance of a lecture" delivered by him at the Royal College of Physicians of England, "at the request of the President and Council." In appendices to this lecture will be found an important extract from the proceedings of the Royal Sanitary Commission of 1871, and an important "Memorandum in reference to the Establishment of a Qualification in State Medicine in the University of Dublin," by Dr. Stokes, dated January, 1870.

These documents may be considered as the foundation upon which the *Profession* of State Medicine in the United Kingdom has been gradually developing for upwards of 20 years, and I much fear these foundations have been sapped by the action of the General Medical Council. Nearly all authorities seem to concur in the view that there should be two grades of qualifications in State Medicine—one for those district poor-law medical officers who necessarily must combine many of the duties of health officers with those of attendance on the sick poor; such a qualification should also be required of officers of the Navy and Army, and resident officers of all public institutions, such as lunatic asylums, prisons, hospitals, &c. The Royal Sanitary Commissioners, in that portion of the Report quoted by Dr. Acland in his appendix, say:—"This being so, the basis of the public health staff in this country may with great advantage (in consequence of the universal presence of the sick poor) be the body of men now known under the name of *poor-law medical officers*, a staff of public officers paid out of public funds. Of these there are already in England alone about 4,000. After attentively considering this central point, and having regard to the three principles already enumerated—viz., universality, efficiency, and economy—we find ourselves driven to the conclusion that they form the most eligible basis of sanitary executive in Great Britain.

"We admit, *in limine*, that an objection may be taken by some who associate the idea of inferiority with the name of parish

doctor. But such notions should be set aside as now no longer applicable, either to the medical profession or the administration of the poor-law. This gigantic instrument of social charity, if chargeable with some evils, is at least unique in the good which it attempts.

"We have in other parts of our Report enumerated the various arguments for and against the expediency of taking counties, unions, parishes, and river basins as the sanitary limits or areas, and we have freely discussed the question of the fitness or unfitness of the county boards, unions, and parish officers and their commissioners for the regulation of this part of the local arrangements of the kingdom. Of the propriety of the selection of the poor-law medical officers as the permanent agents in the disease-prevention organisation we have no doubt. The following reasons for this conclusion will also help to illustrate their duties, and their place in the general health administration :—

"1st. These officers exist already and everywhere.

"2nd. They are intimately acquainted with the habits of the poorer classes, the classes which most need assistance. They are themselves fairly educated and belong to that part of the middle class which combine sterling practical habits with considerable culture, and live a life which itself is a perpetual education."

The Commissioners go on to point out the various good qualities for the service possessed by the members of the Poor-law Medical Service, and proceed to define the duties which the Commissioners think they should perform, and then proceed to deal with the experts or superior officers, who should be highly and specially qualified in State Medicine, and devote their whole time to their duties.

I believe it will be found absolutely necessary to have these two classes of offices and two classes of diplomates to fill them. The Committee of the General Medical Council thought so too in the year 1869, and so did the Council of that day. So evidently did Dr. Stokes when he drew up his memorandum for the *higher*, not, be it remarked, the lower qualification. I am sorry to say I think the authorities of the University of Dublin seem inclined to take the lower as their ideal instead of the higher. They have reduced

the standard required by candidates and have accepted the regulations of the General Medical Council.

If the General Medical Council desire to create experts devoting their whole time to the work of medical officers of health over large areas, or to act as district inspectors, their programme is miserably insufficient. If it is for a district officer their standard is too high, as I have already pointed out. The Council have, as is too common in these days, accepted a compromise in order to get out of a difficulty, and thus required conditions which, in my opinion, are suitable neither for a higher nor for a lower qualification in State Medicine, and which I trust will not be received, except after careful inquiry, as a qualification by any body wishing to appoint a fully qualified expert as medical officer of health over a large and populous district.

I am afraid it is now too late to alter the ill-judged action of the General Medical Council, but I think the universities and colleges will find at no distant date that a higher qualification than that represented by the minimum of the General Medical Council will be necessary, in order to avoid a complete loss of confidence of the public health authorities in State Medicine diplomas as qualifications for the higher duties of medical officers of health.

It will naturally be expected that I should suggest some system of examination or curriculum of study, and definition between the two diplomas which I have suggested as a higher and lower qualification in State Medicine.

I shall take the lower qualification first; for this, I am of opinion, there should not be any curriculum of study required. I would allow the candidate to present himself for examination at any time after he had completed his regular medical curriculum, and would confer his diplomas at the same time, or immediately after he had received those in medicine, surgery, and midwifery required for registration under the Medical Acts. I would require the candidate for this diploma to answer at a special examination comprising hygiene, chemistry as applied to sanitary questions, elementary sanitary engineering—to be defined by syllabus; to show a slight knowledge of the law as regards nuisances, the

spread of infective disease, &c.—all defined by syllabus. This qualification might be termed a certificate in sanitary science, and should be a registrable qualification.

For the higher qualification, I would require the candidate to be a registered medical practitioner of two or three years standing—I prefer three years. He should produce evidence of having studied Law, Engineering and Architecture, Vital Statistics, Geology and Meteorology, as applied to State Medicine.

The examination should extend over several days, and I think the classification of subjects of examination adopted by the University of Dublin may be taken as a good one. This qualification might be designated a Diploma in State Medicine.

As already pointed out, I think all poor-law medical officers and resident medical officers of public institutions should be required to possess the minor qualification, and I do not consider any one should be permitted to act as an inspector in a medical capacity, such as Local Government Board, Factory, Lunacy, or Prison Medical Inspector, without possessing the higher qualification; neither should any one be permitted to act as a superintendent medical officer of health or medical officer of health of a large and populous district—and I think every district should be large and populous—without the possession of the higher qualification.

ON BOARDING-OUT AS A MEANS OF PROVIDING FOR THE CHRONIC INSANE OF THE POORER CLASS.

By CONOLLY NORMAN, F.R.C.P.;
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[Read in the Section of State Medicine, April 25, 1890.]

Insanity differs from other diseases in this respect, that its treatment involves social questions of wide reach and considerable difficulty.

Deprivation of the full enjoyment of liberty is an unavoidable preliminary to dealing with ordinary cases of mental disease. Frequently, also, society demands the curtailment of the lunatic's freedom, with very little regard to the question of his treatment.

Unfitness to fulfil social duties, and incapacity to take his proper stand and make good his fight in the struggle for existence, tend to bring upon the lunatic pauperism as well as restraint. This, of course, is specially so among the poorer classes; and as insanity is often a disease of very protracted duration, we find ourselves face to face not merely with the difficulties arising from the deprivation of liberty, whether for the patient's good or that of his fellow-citizens, but also with the problem of state provision for one who is unable to provide for himself.

Thus the question of the mode in which the insane ought to be dealt with becomes one of the most important in State Medicine. In no department has more been done through the labours of philanthropic physicians. We have little to show that has been effected through the mere use of drugs, but there are few branches of medical work in which of late years more advance has been

made towards the ultimate end of all our toil, the prolonging of life, and the alleviation of suffering.

For a large number of the insane we may set it down without hesitation that asylum treatment is a necessity. It may be said, and I think it has been said, that nobody need be sent to an asylum if his friends are rich enough to keep him at home, if they can provide him with a separate house or a separate wing of a house and plenty of attendants, and so forth. This, however, is only a verbal question. The private house kept for the rich lunatic becomes, when he is detained there, an asylum in everything except in name. But for the great bulk of the middle classes and for all the poor—who more particularly interest us—such an arrangement is impossible. For the latter, treatment in bulk, treatment in a large public institution, in a place of restraint, whether it be a workhouse wing, or what we call an asylum, has been the only course open.

An asylum officer is not likely to minimise the advantages to the poor which our public asylums offer, and perhaps it is unnecessary here to dwell upon them; it is rather to the other side of the picture I would draw your attention. This is sometimes overlooked. A lunatic is troublesome. He is sent to an asylum. The public are satisfied that he is in a way well cared for; they are, indeed, often generously anxious to secure good treatment for him; and—out of sight out of mind—he is dismissed from recollection.

The forgotten lunatic, however, often entertains the ordinary feelings of humanity quite as acutely as those who have retained their mental balance. The best conducted asylum is yet in some sense a prison, and imprisonment, "disguise itself as it will, is still a bitter draught." There is, and always must be, a want of human interest about our endeavours to amuse and occupy our asylum inmates. The dreary sameness of these poor people's lives certainly often leads to the supervention of intellectual degeneration earlier than it might occur under more favourable conditions. The affections are checked and chilled by the same unnatural life that dulls the intellect. Who can love his jailer?—in which light unhappily our patients usually regard those who have charge of them.

Though otherwise applied, the poet's words well describe the lunatic in an asylum :—

> "The narrow brain, the stony heart,
> The staring eye glazed o'er with sapless days,
> The long mechanic pacings to and fro,
> The set gray life, and apathetic end."

It becomes, then, a question worthy of the attention of the physician whether, in any considerable proportion of cases, a mode of dealing with the chronic insane poor might not be adopted which would save them from the life of incarceration which they would otherwise have to endure.

This the system of boarding-out has been held to offer, and it therefore appears worth our while to consider what its claims are, and what it has done.

That which for brevity's sake we call "boarding-out" may be described as being the management of the insane in private dwellings, with state support and under state supervision. Lunatics have been domiciled in the homes of sane members of the population on a large scale, and with public recognition, in two countries: Belgium and Scotland. I do not think that any experiments made elsewhere need engage our attention, in view of the results of Belgian and Scotch experience.

First let us consider what has been done in Belgium.

The Belgian boarding-out system centres at Gheel, a village in the Campine or Kempenland, about twenty-five miles east of Antwerp. Gheel has a special interest for Irishmen. St. Dymphna, the daughter of an Irish king, Daimhen of Oriel, was martyred at Gheel in the seventh century. Her shrine became a place of pilgrimage for those who suffered from mental disturbance. The villagers boarded these poor folk, and thus gradually the so-called insane colony arose.[a]

[a] In earlier times and among unsophisticated populations the ideas of shame and concealment with which we are now so familiar in connection with insanity do not seem to have attached to that affection. Lunatics went voluntarily on pilgrimages to various places reputed for the cure of their ailment. Such a place of pilgrimage was the Well of Saint Maree, on an island, Innis Maree, in the loch of that name, in Rosshire, Scotland. In our own country, a beautiful valley on the shore of Dingle Bay, in Kerry, called Glen na Galt, used to be

The institution is now recognised and regulated by law, and receives indigent lunatics from a large section of Belgium, as well as paying patients from all countries.

The Kempenland is arid and sandy, and can be only made fertile by assiduous culture. The peasantry and the poorer lunatics who board with them are chiefly employed at tillage, the women at dairy work, and soforth (the women in Belgium work largely in the fields also).

According to the latest statistics at my disposal the village of Gheel contains 5,000 inhabitants, while Gheel taken together with the neighbouring villages, which form part of the lunatic-receiving district, has a total population of 11,000. This gross number occupy 3,025 houses, of which 1,100 contain lunatics. The area of the district is 24,000 acres, the circumference about twenty-six miles. There are altogether 1,653 patients, out of which number 1,460 are indigent.

A central hospital containing sixty beds exists in the village of Gheel.

Five medical officers reside in the district, of whom the Director (Medical Superintendent) lives in the hospital.

Certain classes of cases are excluded by law: the homicidal, the suicidal, the incendiary, those apt to offend against public decency, and those for whom continuous restraint is deemed necessary.

Occasional restraint is admitted to exist, though it is perhaps rarely resorted to. What would in these countries be regarded as a form of neglect worse than occasional restraint appears to be habitual. My friend, Dr. Hack Tuke, notes that when he visited Gheel in 1886, he found in one cottage "two female idiots and a

much resorted to by the insane. When I visited Glen na Galt with Dr. Hack Tuke, in the year 1885, I was assured that within thirty years a lunatic (evidently a melancholic, to judge by the description given by my informant, who had been an eye-witness) had come to the valley, built himself a little hut, and lived there till he recovered. Much more recently a lunatic had escaped from the County Asylum and sought shelter in Glen na Galt, where he was recaptured. I cannot find that there was ever any shrine in Glen na Galt. There were two holy wells, now filled or dried up, but no traditional or folk-lore explanation seems to survive to show how they originally became places of pilgrimage for the mentally afflicted.

dement. The former were provided with pierced chairs in the room in which the family lived."

Dr. Tuke also observes the frequency with which lunatics are boarded in wayside taverns (estaminets). One house which was visited was an estaminet. In the common room two insane women lived. One was tranquil, one restless. Men dropped in, drank their glass of beer, and played cards in this room.

At the same time, the better class patients at Gheel are treated with startling freedom. Some of them seem to eat and drink when they please, and what they please, to sleep or idle at their own sweet will; some appear to go to Antwerp when they choose, and do not always spend their holiday there in the most edifying manner.

Within the last ten years an erotic female and an epileptic male being located in the same house, pregnancy and infanticide followed. While the official records of illegitimacy are certainly very low, it has always struck foreigners that with the lax custody maintained over the less severe cases of mental disease, there must be a good deal of immorality. Dr. Hesse tells us that the director, Dr. Peeters, states that one pregnancy occurs in about every five years, and that he himself regrets the insufficiency of the arrangements for erotic women, which Dr. Hesse truly calls deficient and culpable.*

Classification and Cost.—Indigent patients are divided into three classes, and are paid for according to classification: for

The clean ...	...	8d.	per diem	is paid.
The semi-dirty	...	9d.	,,	,,
The dirty ...	...	10¾d.	,,	,,

While we must give to Gheel the credit of having maintained for many centuries the traditional practice of treating the insane with an amount of freedom from incarceration unknown anywhere else in the world, and thereby done a great and instructive work,

* The last-named writer, while praising many points in Gheel, comments rather severely on the discomfort and wretchedness of many of the cottages in which lunatics are boarded in the more remote and poorer parts of the Gheel district. This is important to note, the witness being a German physician, who cannot be supposed to be biassed by the luxury of English asylum arrangements. Indeed Dr. Hesse's paper is throughout most temperate and judicious.

it would certainly seem that the Gheel system is not applicable elsewhere, and that there are faults of the gravest nature to be found with the Gheel practice.

The very origin of Gheel marks it as place apart. It had its foundation in the primitive piety of early ages. For at least seven hundred years (the Church of St. Dymphna dates from the 12th century), pilgrimages have been made to Gheel, and the place has been a home for the mentally afflicted. Generation after generation of the people has grown up to the care of lunatics. The management of the insane has become, so to speak, an hereditary tact with them. This could no more be exported than the other traditions of Gheel.

There does not seem to be a due discrimination in the selection of cases for treatment. The custom of boarding-out dirty and semi-dirty patients cannot be too strongly condemned. I hope we shall never see in this country a dirty patient confined in a pierced chair in the very midst of a sane family.

On the other hand, there cannot be a doubt that freedom in a certain class of patients is carried too far.

The cost, considering the cheapness of living in Belgium, is not remarkably low, and when the charges of the central hospital are taken into account, it would appear to be very little less than that of the ordinary asylums.

A system of managing the insane poor in private dwellings has grown up in Scotland, which, while in some respects resembling that in use at Gheel, seems to offer a better model, and to be free from many of the disadvantages of the Belgian mode.

On the 1st January, 1889, there were under the jurisdiction of the Scotch Lunacy Commissioners, 10,004 pauper lunatics. Of these, 2,297 were maintained in private dwellings. The latter number, however, includes lunatics for whom, under the provisions of the Scotch law, an allowance is made to relatives for the purpose of maintaining them at home. These are hardly to be described as boarded-out in the same sense in which that phrase might be applied to the indigent insane at Gheel. Unfortunately the Lunacy Blue Book does not contain any information from

which one can gather how many lunatics are living with relatives and how many with strangers, except in the case of Midlothian. In that county 60 pauper patients reside with strangers, 56 with relatives—respectively, 51·7 and 48·3 per cent. of the total number residing in private dwellings.

The system now at work in Scotland is, like the Belgian, to a certain degree a modification and development of a primitive plan, the object of which was not at all identical with present results.

I have authority for saying that "the providing for harmless and incurable insane persons in the houses of relatives scattered all over the face of Scotland is regarded as the perfect carrying out of the idea. But many things make it difficult to place such insane persons with relatives. Very often there are no relatives with whom they can be placed, and quite as often when relatives exist they are not suitable guardians, because the insanity which is actual in the persons to be boarded-out is potential in the relatives, and shows itself in dissipation, thriftlessness, comfortless surroundings, poverty, untrustworthiness, and other such things, which render them unfit to be guardians. Therefore it became necessary to find guardians among persons who were not related to the patients—who were 'strangers,' as we call them. When one, two or three patients were thus sent to a guardian who was a 'stranger' to them, and were seen by the other people in the village in which the guardian lived to be quite harmless, applications were made by the other villagers having spare accommodation in their houses to get boarders." (Sir Arthur Mitchell.) Thus arose in Scotland a number of centres in which boarding-out is now extensively practised, so-called "insane colonies" (a term, by the way, which is not recognised by the Commissioners). With these, it must be always remembered, co-exists the family care of the insane by their own relatives, under legal supervision and with the aid of public money.

The tendency to concentration of cases in the care of strangers has led to a large number of lunatics being thus boarded-out in certain villages in the counties of Fife and Perth.

The following villages contained, at the close of the year 1887,

the number of patients placed opposite their names. (See Report of Deputy-Commissioner Lawson):—

Fife—

Kennoway - - - - -	65
Thornton - - - - -	29
Star - - - - - -	40
Auchermuchty - - - -	14

Perth—

Gartmore - - - - -	50
Scone - - - - -	12

The number of the sane population of those villages I do not know, but they are small country villages, situated in agricultural districts.

With regard to the question of how the general system works in Scotland, it may be briefly said that it meets with the unmixed approval of the Scotch Lunacy Board. Each annual Blue Book for many years past contains important reports describing and commending the work done in this department. These reports form a mass of literature exhibiting a most ably presented case in favour of boarding-out. The system has obtained the commendation of the majority of the asylum physicians in Scotland, and would certainly appear to have won the confidence of the governing bodies of asylums and of the general public.

What advantages are claimed for boarding-out in Scotland?

Economy.—First, with reference to cost, which of course seriously affects the feasibility of any scheme of the kind.

It is not easy to calculate exactly the cost of these cases, which differs in different localities, and which will naturally be affected by any material difference in the conditions of various districts. The calculation made by the Committee of Woodilee Asylum, Glasgow, when they reported on boarding-out in the year 1885, was that each patient in the asylum cost 13s. 7·35d. per week, while those boarded cost 8s. 9d.

In the Scotch Blue Book for 1888 a table is given (p. 41) showing the comparative cost of daily maintenance of pauper lunatics in asylums, in special wards of poorhouses, and in private dwell-

ings. In asylums the average amounted to 1s. 4¾d.; in poor-houses to 1s. 0¼d.; in private dwellings to 10¼d.

The average for asylums, as Dr. Turnbull points out, does not include a charge for "rent," the erection and keeping up of buildings, &c., so that it is greatly an under-estimate.

In Fife, which is mainly an agricultural county, the usual charge for a male patient boarded with strangers is 7s. per week, and for a female 6s. per week.

In this connection it is only necessary to remind my hearers that these patients work for their hosts, and that the profits of the latter consist of the difference between what the lunatic costs them and the amount they receive, *plus* the value of his or her labour. Thus the persons receiving boarders can afford to accept rates which at a first glance might seem insufficient.

Probably we must also, in dealing with the Scotch statistics, take into account the fact that the apparent average cost is reduced by the number of patients boarded with relatives. A lunatic's own family will be thankful to receive for his maintenance a much smaller sum than they would care to accept for the support of a stranger.

Advantage to Patients.—To my mind the chief credit of the boarding-out system is this: that it relieves from the misery of imprisonment patients whose mental state does not necessitate the infliction of that added suffering. I agree with the Scotch Commissioners in thinking that it is not right to keep a patient under asylum restraints and discipline who does not need them, and who would be happier in the enjoyment of the greater freedom which he would have under private care, when the latter can be properly secured.

Besides being pleasant, it is undoubtedly beneficial to a large number of chronic lunatics to be again enabled to return to a certain degree of liberty. Of course the lunatic registered and boarded-out is not absolutely a free man. But taking the case of an artisan or agriculturist boarded with people of his own class, he is in a home-like place with home-like surroundings, and the affections and interests of home can again spring up in him.

That this may actually affect the mental state most beneficially,

is shown by the fact that a not inconsiderable proportion of lunatics boarded-out in Scotland annually recover or cease to be chargeable to the rates—*i.e.*, become self-supporting.

Relief to Asylums.—There is a third advantage—namely, the removal from asylums of the large class of chronic cases that tend to accumulate—to increase expense, to distract the energies of the staff, and to occupy space that could better be reserved for recent cases, and cases absolutely requiring asylum treatment. This it is, in fact, which has led the Committees of many of the Scotch asylums to adopt the plan of boarding-out.

Disadvantages and Dangers of Boarding-out.—It should be said that there is another side to the fiscal question. If our chronic incurable patients, who include the majority of our working men and women, are removed from us, it will be necessary considerably to increase the staff of sane workers and attendants. This will not only increase the capitation on a decreased total number of patients, but it will even go far to counterbalance, if it does not altogether counterbalance, any saving effected by the cheaper mode of supporting those who are boarded-out. There is also to be taken into account the expense of supervision for the boarded-out, lay and medical, local and central. Thus it may seem fairly questionable whether any direct monetary gain to the public would result from the adoption of this system. On the other hand, there can be no doubt that what is best for the insane is in the end best for the pockets of the ratepayers, as was long ago pointed out by Sir Arthur Mitchell and others. If patients who would otherwise remain all their lives in asylums, can be rendered self-supporting, as we are told frequently happens in Scotland, money is gained as well as other advantages.

Again, the chronic lunatic is a life-long burden to the State. The best prospects of cure are offered in the early stages of the mental malady. It is then that the patient derives benefit from vigorous treatment in a special institution. It is then that his case requires constant study, that he himself requires minute individual care and incessant attention. It is then that he is benefited by removal from the home surroundings that

are irritating and disturbing to his unsettled mind, and it is then that the injudicious care of over-anxious friends may do most harm. If at this stage he is sent to an asylum, he should receive an amount of personal care which is impossible in over-crowded institutions, that are practically more poor-houses than refuges for the insane. As the result of my own experience, I have come to a very firm conviction, which I lay before you for what it is worth: that on the whole, patients under treatment for acute mental diseases tend to recover just in proportion to the individual personal care and attention they receive, always of course supposing that the attention is that of skilled persons. It is a well-known fact that the event of an intercurrent affection requiring infirmary treatment, often seems to bring about mental improvement. There are probably various factors in this result. One too little noticed element is, I am satisfied, the care and attention which the sick lunatic receives, the very favourable mode in which he is brought into contact with sane minds at a period when care and kindness are likely to reach him.

I am not a believer in "medicine in a bottle," nor do I think that acute mental diseases can be better treated by an obstetrician or an oculist than by an alienist. "So thick a drop serene obscures my orb" to the advantages of modern "progress," that I do not think that every medical practitioner who has *not* studied insanity is better qualified to treat it than he who has. Nevertheless, I have no hesitation in saying that our asylums should be organised, first of all as hospitals—not, indeed, gynæcological hospitals, or ophthalmic hospitals, or even general hospitals, but as psychiatric hospitals—hospitals for the cure and treatment of that class of diseases whose victims they have been built to receive. And this connotes a large, an active, a carefully-selected, and a carefully-trained staff.

If, then, the asylum staff must be increased when its chronic patients are boarded out, and thereby an immediate expense is incurred which swallows up the profit obtained by the cheaper method of dealing with the latter, we have the satisfaction of [illegible]ing that the money is expended at the right end.

We appear to have something of this kind going on in Scotland at present. Several of the Royal Asylums, anxious to perform to the best advantage their function of hospitals for the insane, have been compelled to raise their charges against the parochial authorities for pauper patients. This has induced the parishes to actively take up boarding-out for their chronic cases. The increased expenditure in one case is balanced by the saving in another, or there is even some slight saving on the whole. The result is achieved, in the opinion of the Commissioners, without having caused to the chronic, incurable, and harmless class any injury or loss of comfort or happiness.

The *dangers* to be guarded against in boarding-out may be briefly said to be:—

1. Risk of accidents generally, including what are somewhat euphemistically called "sexual accidents."

2. Relapse, with its consequent inconveniences and risks.

3. Neglect, cruelty, ill-treatment, unjustifiable detention by sordid and unscrupulous guardians.

Proper selection both of patients and guardians, and a carefully organised system of supervision by authorities entrusted with sufficient power to make themselves respected, will go a long way to obviate all these.

During the year 1888, we learn from the Scotch Blue Book, only eight accidents (mostly broken bones or burns) occurred among the boarded-out. Five of those injured were epileptics who hurt themselves in a fit. None of the accidents were serious. There was no case of pregnancy recorded. Twenty-two escapes occurred—that is, twenty-two patients left their guardians not to return. Out of 1,224 patients visited by him, Deputy-Commissioner Fraser notes that forty-three were removed to asylums during the year, of whom, however, only 16 were sent in consequence of being mentally unfit to remain in private dwellings, or being unsatisfactorily provided for. It is interesting to observe that Dr. Fraser records 36 patients as having recovered, and 9 as being removed from the poor roll. In the previous year (1887) two sexual accidents (pregnancies) are recorded. It is only fair to

say that one of them occurred in a patient boarded with her own mother, who might reasonably have been supposed to be an efficient guardian in this particular at least.

The proportion of patients who can be boarded out.—The proportion of the pauper insane under the jurisdiction of the Scotch Lunacy Commissioners during 1888, confined in asylums, in the special wards of poorhouses, and boarded in private dwellings, was respectively—

Asylums - - -	68 per cent.
Poorhouses - - -	9 „
Private dwellings - -	23 „

Dr. Turnbull has calculated that about 28 per cent. of our pauper insane could be provided for by boarding out, and he shows that the city parish of Edinburgh has kept up about that proportion for several years. I express my own notion diffidently, not having had practical acquaintance with the working of the system, but I would incline to consider the latter figure as rather too high, especially as I would differ from the Scotch authorities by excluding epileptics, save such as could be "boarded out" at home. The proportion of registered lunatics which I think it would be fair to calculate could be provided for in private dwellings in any district not presenting special difficulties in boarding-out would be about 20 per cent.

The classes of patients who are suitable for boarding out are, generally speaking—

1. Milder forms of idiocy or congenital weakmindedness.

2. Cases of dementia consecutive to acute mental diseases. These are the cases that accumulate and crowd up our asylums. There can be no doubt that a large number of these could safely be at large, under such supervision as a judiciously worked system of boarding-out would afford.

3. Mild delusional cases in which, though the mind is permanently warped, experience has told us that the patient retains sufficient self-control to be fairly trustworthy.

Unsuitable cases are—

1. Epileptics generally.

2. The actively insane and recent cases; also all cases who have not been under skilled observation for some time.

3. The suicidal, homicidal, and those with any marked dangerous tendencies.

4. Those who entertain sexual delusions, or exhibit any tendency towards sexual excitement.

5. The dirty.

Guardians.—A proper class of patients having been carefully selected, great care must be exercised in the selection of guardians. Boarding-out can nowhere so well be carried out as in an agricultural district.

I perfectly agree in the remarks made by Dr. Lawson with regard to the social status of the guardian. It ought not to be above that of the patient. The object to be aimed at is to provide a *home* for the lunatic. He should associate, so far as his mental condition will allow, with all other members of the household, and should never be allowed to feel that he is a mere servant. If this requirement be not borne in mind, boarding-out will become mere farming-out, and the whole arrangement will end in a vile system of white slavery.

Inspection.—The next point that arises is suitable inspection. In Scotland this is provided for by special statutory enactment. All houses receiving boarders are licensed, and are visited, under careful regulation, by the Inspector of the Poor (relieving officer), and the Parish Medical Officer, as well as by the Assistant Commissioners, whose duties largely are to visit the boarded-out, and by the Commissioners themselves.

The necessity for a due inspection will be abundantly clear to anyone who has an opportunity of consulting Sir Arthur Mitchell's book on "The Insane in Private Dwellings," published in 1864. This work, which served as the foundation of the modern boarding-out system in Scotland, gave a terrible account of the neglect and ill-usage that had existed among lunatics in that country who were boarded out. The genius of its author is well shown by the discrimination with which he saw the advantages of the system amidst the many abuses it then presented, while his courage is

attested by his resolute refusal to understand the difficulties of the task to which he set himself till success had crowned his efforts.

Similarly with Gheel. The pious sympathy that had animated the inhabitants who first acted as hosts to the afflicted visitors had about the middle of this century died quite away, and the most sordid commercial views predominated. The famous alienist physician, Guislain, then Medical Superintendent of the Asylum at Ghent, visited Gheel, and exposed, in scathing terms, the abuses that had sprung up. Government intervened, and took the whole colony under State supervision. In 1851 the first Medical Director, Dr. Parigot (well known as the author of "L'Air libre et la Vie de Famille dans le Commune de Gheel"), was appointed. Dr. Parigot was succeeded by Dr. Bulckens, and he again by the present able Superintendent, Dr. Peeters. The colony has remained under State regulation and medical management; and although, as I have pointed out, many things still exist which one would not wish, progress has been steady, and improvement immense.

SECTION OF ANATOMY AND PHYSIOLOGY.

SIMPLE UNILATERAL (SIGMOID) KIDNEY.

By AMBROSE BIRMINGHAM, M.B.

[Read in the Section of Anatomy and Physiology, January 3, 1890.]

Sigmoid kidney is to be looked upon, owing to its rarity, rather as an anatomical curiosity than as an anomaly likely to come under the notice of the operating surgeon. Nevertheless, since the kidney has come in recent days so thoroughly within the treatment of the surgeon, and since its ablation is now-a-day a frequent event, all anomalies of the organ have gained in interest, and particularly those conditions in which there is but one separate kidney present.

Single kidney may be due to three different causes:—

(1) Congenital absence of one kidney.

(2) Atrophy of one kidney.

(3) Fusion of the two kidneys.

The fusion of the two kidneys may take place to varying extents. According to the degree to which union has taken place we recognise three varieties of this condition. If the fusion be complete the result is a "disc-shaped kidney." On the other hand, if the union be slight, and only between the lower extremities of the two organs, there is formed a "horseshoe kidney." Intermediate between these two, in the degree of fusion, stands "sigmoid kidney."

Disc-shaped kidney is placed in the middle line. Horseshoe kidney has one limb of the shoe on each side of the vertebral column, and the connecting band between the lower ends of the two running across the middle line. In sigmoid kidney there is an entirely different condition of things, for here one kidney (the

right usually) is transferred to the opposite side of the middle line, where it is joined on to the lower and inner part of the left organ; it is thus a form of unilateral kidney.

As to the frequency of these conditions, I have only come across records of two cases of sigmoid kidney—one by Dr. Coupland (*Guy's Hospital Reports*), the other by Dr. G. Broesike (*Virchow's Archives*, November, 1884), both quoted by Mr. Morris in his excellent work on "The Surgery of the Kidney." Mr. Morris also tabulates the *post-mortems* of several London hospitals, with the following results:—In 14,318 cases examined there were found 9 horseshoe, but only 1 fused (sigmoid?) kidney.

During the winter I met a well-marked case of sigmoid kidney in a subject undergoing dissection; I had it carefully dissected; the following is the condition of the parts made out:—The left kidney was nearly normal in size and position; it measured 3¼ inches in length and 2 inches in breadth; it extended from the upper part of the twelfth dorsal vertebra to the upper part of the third lumbar. The left supra-renal body bore the usual relation to its kidney. The hilum was a large, irregular oval depression on the anterior surface, thus complying with the rule that when fusion of the two kidneys takes place the hilum has a tendency to turn on to the anterior surface; the more complete the fusion the more markedly does the hilum come forward. One artery, with the origin of a normal renal, broke up into branches rather earlier than usual, and entered the hilum; three veins made their exit from it; these united about 1½ inches from the hilum to form a normal renal vein. At the hilum the vein lay most anteriorly, the artery intermediate, and the ureter posteriorly, as usual. The ureter ran down in front of the lower portion of its kidney, along the outer part of a slight groove which indicated the junction of the two kidneys.

The lower kidney, which we must regard as the displaced right organ, was joined by its upper end to the lower and inner part of the left; a slight groove on the anterior surface of the fused mass and a notch at either border indicated pretty clearly the line along which the fusion took place. This right kidney inclined down-

wards and towards the mesial line, extending from the lower border of the second to the disc between the fourth and fifth lumbar vertebræ, and approaching the aorta near its bifurcation; thus the mass was remotely sigmoid-shaped.

The hilum was circular, 1½ inches in diameter, and placed on the right side of the anterior surface. The ureter came out of hilum in front of the blood-vessels as three branches, which united ¾ inch below the lower end of the kidney. The vein was single and small; it passed out of the centre of the hilum. The arteries were three in number—one came from the inner side of the *right* common iliac, a short distance from the bifurcation of the aorta, it crossed the left common iliac, reached the hilum, ran along its lower margin, and then turned up by its left lip, approaching a similarly disposed branch of the highest of the three renal arteries; together they encircled the hilum, except at its inner part. The second artery arose from the aorta 1½ inches above its bifurcation, and ran to the posterior aspect of the lower end of the organ. The third, and largest, artery arose from the aorta ½ inch above the last; it gave off a few branches to the inner border, but the greater part ran round the upper and outer margins of the hilum, approaching the similarly disposed branch from the common iliac previously described, and giving off branches on its way which penetrated the substance of the kidney.

The left ureter, as already pointed out, passed down in front of the lower end of its kidney, and then ran a normal course to the bladder. The lower or right ureter came out of the middle of its pelvis, passed down in front of its kidney, over the left common iliac artery, crossed the middle line at upper part of pelvis, running nearly parallel to right common iliac, and thus reached a normal termination at the right side of the base of the bladder.

The left supra-renal capsule was normally placed; the right lay against the right crus of diaphragm at the usual level, and did not seem to be affected in the slightest degree by the absence of the corresponding kidney.

The generally received opinion as to the production of fused kidney is, I think, that each kidney is developed separately to a

certain point, then the two come into contact for some unaccountable reason, and the lobules of the two unite. According to another view the organ is developed as one single mass, and at no time are the two kidneys separate.

There are a few points of peculiarity in the above case. First, the blood supply of the lower division of the mass—the right kidney; one of its arteries came from the right common iliac, and appeared to be dragged across from the right to the left side. Secondly, the ureter left the hilum in front of the vessels contrary to the usual arrangement. Lastly, the position of the hilum was that of the hilum of a right kidney reversed—namely, the hilum was near the right border instead of the left. These relations suggest a transference of the right kidney (with a rotation on its long axis) from the right to the left side of the vertebral column after its vascular connections had been established. They point decidedly to the first of the two theories mentioned above as the one which accounts for the condition of fused kidney most fully.

OPERATIONS ON THE BRAIN.

By ALEC FRASER, M.B.;
Professor of Anatomy, Royal College of Surgeons.

[Read in the Section of Anatomy and Physiology, January 3, 1890.]

In this paper I have passed over the description of the methods which I employed for preparing and embedding the heads, as well as those followed for serial and composite photography, and have restricted my remarks to the fixing of the tapes on the cranial vault and to the relations which the cortex and the central parts of the brain bear to the scalp surface. Those interested in the methods employed, or in descriptive and bibliographical detail, can refer to my book.

FIXING THE TAPES.

The tapes were hand-made, and divided into inches and half-inches. I am conscious of the drawback which the adoption of these measurements entails, but it is not insuperable, as they can readily enough be turned into centimetres.

The primary tape, with which all the others are connected, is the circumferential, passing horizontally round the vault of the head, from the root of the nose, between the eyebrows in front (glabella), to the maximum occipital point behind (should there be any difficulty in locating this latter point, take it as one inch above the external occipital protuberance), and thence round to the starting-point.

This tape, giving the circumferential measurement of the head, is then divided into four equal parts; if it is, say, 23 inches, then the subdivisions will be marked at $5\frac{3}{4}$, $11\frac{1}{2}$, and $17\frac{1}{4}$ inches; the starting-point and the $11\frac{1}{2}$ inches are to be the poles where all the tapes running longitudinally are to meet; while the $5\frac{3}{4}$ and the $17\frac{1}{4}$

inches are to be the meeting-points of the tapes running transversely.

Practically I place these on the heads as follows. I do not start with running a tack through the beginning of the first inch of the circumferential tape alone, but, in addition, through all those that are to run in a longitudinal direction—that is, three if I am making the series of dissections from the top, the back, or the front of the head and neck, or two if I am working from the side. I drive this tack, with these three or four tapes on it, through the scalp into the frontal bone at the point previously mentioned, then I run the circumferential tape laterally round the vault, passing over the posterior fixed point as also previously mentioned, and secure this tape by driving a second tack through it into the frontal bone at the starting-point; then I take the other three (or two) tapes and run one of them along the median longitudinal line of the head from front to back, and the remaining two (or one) laterally, so as to divide each lateral half of the median transverse tape into two equal parts, the three (or two) longitudinal tapes being finally fixed by driving a tack through them and the centre of the circumferential tape into the scalp and the occipital bone behind.

I now proceed to place the transversely directed tapes, the median one running in a plane vertical to the circumferential tape, and passing through the ends of the first and third quadrants of this tape; this generally passes over the median longitudinal tape slightly in front of its centre, the posterior segment of the head being nearly always larger than the anterior.

I fix this tape by running a tack through it and the median longitudinal into the bone where the two cross. I then divide the anterior and posterior segments of the vault mapped out by this median transverse tape and the circumferential into equal parts along the median longitudinal tape; and at these points I fix the anterior and posterior transverse tapes by running a tack through them and the median longitudinal one into the bone; the measurements on these three transverse tapes run (from their fixed points on the median longitudinal tape) from the beginning

of the first inch on either side downwards towards the circumferential tape. The free ends of these three transversely directed tapes are then secured by passing a tack through them at the termination of the first and third quadrants of the circumferential tape, and the fixing of the tapes is complete.

A glance at Photographs 1, 28, 33, 35, and 42 will enable you to understand how these lie, better than any description of mine.

By means of the longitudinal tapes the vault above the circumferential one is divided into four segments, which are again subdivided by the transversely directed ones into a series of four right and left median spaces, and into a right and left lateral series of four. These I shall always speak of, passing from the frontal to the occipital end of the head, as the first, second, third, and fourth median spaces right or left, and the first, second, third, and fourth lateral spaces also right or left.

I shall also always speak of the tapes as the circumferential, the median, and the right and left lateral longitudinal, the median, and the anterior and posterior transverse, it being understood that the planes of the right and left lateral longitudinal, and those of the anterior and posterior transverse have an oblique as well as a longitudinal and transverse direction.

RELATIONS OF THE EXTERNAL SURFACE OF THE BRAIN TO THE SCALP.

Before entering upon the description of these relations, I may state that the key-note of all the remarks which are to follow is this—that for normal heads after birth the position of any structure of importance in their cavities varies proportionally with the variations of the circumference as given by the circumferential tapes of the respective heads.

To represent these from every point of view, I have selected the composites showing views of the skin and brain from eighteen different heads. Photographs 14 to 17 and Photograph 27, for the adult side series, and the same series in children, Photographs 18 and 19. For the series from the top the relations are shown in

Photographs 28, 29 and 32 in the adult; and for children in Photograph 31. In the back series the relations are shown for the adult in Photograph 35, and for the child in Photograph 37, while the relations from the front are shown (but not very well) in Photograph 42. The relations of the circumferential tape to the underlying parts of the brain can be read from Photographs 14 to 19, and in 35, 37, and 42. It lies underneath the frontal lobe, save a portion of its orbital surface, meets the anterior extremity of the temporo-sphenoidal lobe at measurements varying in the adult from 3¼ to 4 inches, Photographs 14 to 17, and in children from 2½ to 2¾ inches, Photographs 18 and 19; that is, at distances varying with the circumference of the respective heads. The anterior end of this lobe sinks into the fossa of the base of the skull in front of the petrous portion of the temporal bone to the extent of one inch below the tape, the lower surface of the lobe, as seen in Photograph 42, being on a level with the floor of the orbital cavity. The tape then passes along slightly above the lower margin of this lobe at its most curved portion, and also to a greater extent above the lower margin of the occipital lobe, as can be seen in Photographs 14 to 19, 35 and 37.

The relations of the median longitudinal tape to the brain surface are displayed in Photographs 28, 29, 31, 32, and in 35, 37, and 42. These relations require no explanation, corresponding, as they do, to the position of the superior longitudinal sinus.

The vertical relations of the lateral longitudinal tape are seen in Photograph 27, where it crosses in order from behind forwards the superior occipital, the second annectant, and the angular gyrus slightly below its upper border; crosses the supramarginal convolution also, but to a less extent than the angular; then passes over the two central convolutions below their middle, then comes into relation with the posterior portion of the inferior frontal gyrus, and finally crosses the second and first frontal gyri, as shown in Photograph 42. The exact posterior relations of the tape are displayed in Photographs 35 and 37.

The relations of the median transverse tape are seen from the side in Photographs 14 to 17 and 27 in the adult, 18 and 19 in

the child; from the top in Photographs 28 and 29 in the adult, and in 31 in the child: these are fixed and constant for all the series. The tape passes upwards from the circumferential one, over the middle and superior temporo-sphenoidal convolutions, over the Sylvian fissure, then always crosses the lower portion of the posterior central convolution, then the anterior central one, and finally ends in the manner seen in the top views, by crossing the posterior extremity of the first frontal gyrus, slightly anterior to its origin from the median end of the anterior central gyrus.

Those of the anterior transverse tape are seen in the same photographs, and it will be found to pass over parts of the middle and superior temporo-sphenoidal convolutions, the anterior end of the Sylvian fissure, the pars triangularis of the inferior frontal convolution, the middle frontal, and, lastly, the superior frontal, anterior to its centre.

The relations of the posterior transverse tape are also seen in the same Photographs; it passes latterly upwards over the middle temporo-sphenoidal convolution, then over the posterior border of the superior temporo-sphenoidal as this gyrus passes upwards to become continuous with the supramarginal, then over the angular, and its exact median relations can be judged from Photograph 32, where it ends by lying over the superior parietal lobule, about one inch in front of the parieto-occipital fissure. This posterior oblique view has not been represented in the children's heads, but the relations can be judged of from Photographs 18 and 19; neither have the exact vertical relations of the median portion of the anterior transverse tape been shown. That would have required a head embedded up to the level of the posterior transverse tape; the vertical relations of the gyri of the frontal lobe can, however, easily be determined from the side views. All the relations of the various tapes, as can be seen by a glance at the several heads, are the same in the young and in the adult, and exhibit in a very striking manner the proportional relationships of the external surface of the brain in heads varying from 14¼ to 23½ inches in circumference.

Before enumerating the portions of the cortical surface contained

in the median and lateral spaces, into which the scalp surface is divided by the tapes, I shall describe the relations which the central sulcus, the fissure of Sylvius, and the parieto-occipital fissure bear to these tapes.

These relations vary according to the general rule already stated, and can be reached without any difficulty if this is kept in mind.

The median extremity of the central, or Rolando's sulcus, varies on the median longitudinal tapes from $6\frac{1}{2}$ to $7\frac{1}{2}$ inches from the root of the nose (Photographs 28 and 29) in the adult, and in children from $4\frac{7}{8}$ to $5\frac{1}{2}$ inches (Photograph 31).

Its lateral or inferior extremity is always found slightly in front of the median transverse tape, and at a level on the latter varying from 4 to $4\frac{3}{8}$ inches in the adult, and from $2\frac{3}{4}$ to 3 inches in the child, from the middle line of the head (Photographs 14 to 19).

The parieto-occipital fissure varies on the median longitudinal tape in the adult from $9\frac{1}{2}$ to $10\frac{5}{8}$ inches from the root of the nose, and in children from $6\frac{7}{8}$ to $7\frac{7}{8}$ inches, in Photograph 32, for the vertical prolongation of the long axis of the fissure to the scalp; Photograph 35 for the horizontal relation of its extremity, and the top views for the vertical relation of its extremity in the adult; Photograph 37 for the horizontal relation of its extremity to the scalp in the child.

This fissure has not the fixed and constant character of the central sulcus, being often bifid at its extremity, and unsymmetrical as regards position in the two hemispheres of the same head (Photograph 37), as already noted by Hare, so that it cannot be depended upon in the same manner as Rolando's sulcus. It will be noted that the fissure lies slightly anterior to the lambdoid suture, the latter varying on the tape from $9\frac{3}{4}$ to $11\frac{1}{8}$ inches, while the former varies from $9\frac{1}{2}$ to $10\frac{5}{8}$ inches.

The fissure of Sylvius has the course and relations to the transverse tapes shown in Photographs 14 to 19. The middle of its horizontal limb can be found along the median transverse tape, slightly over one inch from the circumferential one, or, if measured from the mid-sagittal line of the head, from $4\frac{1}{4}$ to $4\frac{1}{2}$ inches, and in children from 3 to $3\frac{1}{4}$ inches. The beginning of the horizontal

limb of the fissure can be reached along the anterior transverse tape, $1\frac{1}{4}$ to $1\frac{1}{2}$ inches from the circumferential one, or from the middle line of the head from 4 to $4\frac{1}{2}$ inches, and in children from $2\frac{3}{4}$ to $3\frac{1}{4}$ inches. The posterior tape does not come into relation with the fissure at all.

In consequence of the change in the squamous suture from a linear one in the child to an overlapping one in the adult, the relation of the fissure to the suture, or what is called the suture, is apparently not a fixed one. The line of junction of the upper margin of the squamous part of the temporal with the external surface of the parietal bone has nearly the same position as the horizontal limb of the fissure backwards as far as the median transverse tape; beyond it the suture has no relation to the fissure. In front of the median tape, however, the fissure is practically at the same level as the suture, although absolutely it may either fall slightly above or slightly below the fissure, especially at the position of the anterior transverse tape, this depending upon the amount of overlap of the parietal by the sqamous part of the temporal bone.

It has already been noted that the lower borders of the parietal bones are the comparable parts in the young and in the adult, and that their relations to the underlying portions of the brain are the same, falling in both considerably below the level of the fissure of Sylvius.

I pass now to the enumeration of the cortical contents of the spaces marked off by the tape on the skin. Those of the lateral spaces are seen in Photographs 14 to 19, the median spaces in Photographs 27, 28, 29, and 31; the third and fourth median spaces in Photograph 32, the fourth median and lateral spaces from the back in Photographs 35 and 37, and the first median and lateral spaces from the front in Photograph 42.

A glance at the side or top views in as many different heads, young and old alike, will show that the contents are practically similar in all; in truth, the manner in which the tapes divide the brain into like areas at different periods of life is astonishing.

In the first lateral space are found portions of the superior middle and inferior frontal gyri and the orbital convolutions in

part, a division of the vertical, and the beginning of the horizontal limb of the fissure of Sylvius, the anterior extremity of the temporo-sphenoidal lobe and a small part of the upper and middle temporo-sphenoidal convolutions.

In the second lateral space are found the lower portions of the central and precentral sulci, portions of the ascending frontal and parietal convolutions, the pars opercularis, and a portion of the pars triangularis of the inferior frontal convolution; also the Sylvian fissure in part, and small areas of the superior and middle temporo-sphenoidal convolutions.

In the third lateral space are found the lower part of the post-central sulcus, and part of the posterior central or ascending parietal gyrus, the supramarginal and portions of the upper and middle temporo-sphenoidal gyri, and a corresponding part of the parallel sulcus.

In the fourth or posterior lateral are found the greater portion of the angular gyrus, the terminations of the middle and a part of the inferior temporo-sphenoidal gyri with their continuations as second, third, and fourth annectants, and portions of the superior middle and inferior occipital convolutions and the corresponding sulci.

The contents of the median spaces are illustrated by the top, front, back, obliquely from the side and obliquely from the top views. In the anterior or first median space are found the smaller divisions of the superior middle and frontal convolutions.

In the second median are found the larger divisions of the superior and middle frontal gyri, a part of the precentral or ascending frontal convolution, with the superior frontal and upper ramus of the precentral sulcus.

In the third median are found the posterior end of the superior frontal convolution, the greater part of the anterior and posterior central gyri with the corresponding portion of the central or Rolando's sulcus, a great part of the intraparietal sulcus, a small portion of the supramarginal, with a larger part of the angular gyrus, and the greater portion of the superior parietal lobule. Photograph 32, which was meant to illustrate this and the succeed-

ing median space, shows the variations that may occur; the head was not symmetrical, one of the lateral longitudinal tapes being half an inch longer than the other. This asymmetry is also shown by the convolutions of the adjacent hemisphere, there being four well-marked central convolutions on the one side but not on the other; the intraparietal sulcus being in this manner bridged over by the union of the superior parietal lobule with the angular and supramarginal gyri.

In the posterior median space, Photographs 32, 35, and 37, are found the posterior part of the superior parietal lobule and its continuation, the first annectant; a part of the angular and second annectant gyri; a portion of the intraparietal and its continuation, the superior occipital sulcus; also the superior occipital gyrus and the parieto-occipital fissure.

In this enumeration of the contents of these spaces I have entered into no detail, as the Photographs can be read by those who may glance at them with as much ease as I can do it.

RELATIONS OF THE CENTRAL LOBES AND LATERAL VENTRICLES TO THE SCALP.

The Photographs illustrating these are from 20 to 23, 27, 30, 31, and 38. A glance at the photographs will show the same proportional regularity in the position of the central lobes to the scalp as was the case with the several areas of the external surface of the brain.

The stem or peduncle of the island lies in the first lateral space slightly above the level of the circumferential tape. The entire central lobe, with the corona radiata, occupies portions of the first, second, third, and encroaches slightly on the fourth lateral space. The amount of corona in the latter depends on the obliquity of the incision through its occipital portion. The incision follows roughly the junction of the commissural with the peduncular system of fibres, as well as the curve of the caudate nucleus. This obliquity is somewhat greater in the dissection illustrated in Photograph 21, so that it occupies more of the fourth lateral space than in the other heads. The upper limit of the island from the middle line

of the head falls on the mid-transverse tape at 4 inches in the adult, and at 2½ inches in the child. In the line of the anterior transverse tape at from 3½ to 3¾ inches in the adult, and in the children at 2¼ inches.

The anterior horn of the lateral ventricle occupies part of the first lateral space, the body of the ventricle portions of the second and third lateral spaces, while the cavity of the posterior horn can be readily reached from the fourth lateral space, as I have already indicated when describing this particular serial dissection. Photograph 27, shows the relations of the central lobe and lateral ventricle to the lateral longitudinal tape, as well as the extent of hemisphere left after the curved incisions have been made to lay bare the course and extent of the body and cornua of the ventricle.

The descending horn and the hippocampus major reaches within an inch of the anterior extremity of the temporo-sphenoidal lobe, and can be reached at the level of the circumferential tape. The curve formed by the floor of this horn and that of the posterior one depends chiefly on the amount of curve on the under surfaces of the temporal and occipital lobes, as these lie on the tentorium and the petrous portions of the temporal bones. The amount of curve on these surfaces varies, as can be seen in Photograph 20, where the surfaces are nearly straight, but are more curved in Photographs 21 and 22.

The relations of the central lobes from above are seen in Photographs 30, for the adult, and in 31, for the child. They occupy proportional areas of the second and third median spaces in the three heads illustrated, and extend on either side of the middle line from 1⅝ to 1⅞ inches in the adult, and to 1½ inches in the child.

The position and relations to the scalp surface of the caudate nuclei, the optic thalami, and the other parts in the lateral and third ventricles can be read from the views.

In Photograph 22 the circumferential tape is placed a little too high behind, somewhat over the inch above the external occipital protuberance, and the same in Photograph 25, which is from the same head.

RELATIONS OF THE INTER, MID, HIND, AND AFTER BRAIN, OTHER STRUCTURES, AND THE MEDIAN SURFACE OF THE FORE-BRAIN TO THE SCALP.

These are exhibited from the side in Photographs 24 to 26; the mid and hind brain from the top in Photographs 30 and 31; the hind brain and cervical spinal cord and nerve roots from the back in Photographs 35, 37, and 38; and the central lobes, the inter, mid, hind, and after-brain, with certain cranial and spinal nerves, in Photographs 39, 40, and 41.

The relations of the lower parts of the left hemisphere of the cerebellum are seen in Photographs 14 to 17 for the adult, and in 18 and 19 for the child; in its entirety in Photographs 25 and 26 from the side, and in 39 and 40 from the back. Its lower margin falls 2 inches below the circumferential tapes, or $5\frac{1}{4}$ to $5\frac{1}{2}$ inches vertically from the middle line of the head (not measured on the round); in children it falls $1\frac{1}{4}$ inch below the tape, or $3\frac{3}{4}$ to $3\frac{7}{8}$ inches vertically from the middle line of the head; its upper border rises into the fourth median and lateral spaces, and its vertical height measures $2\frac{1}{4}$ inches in the adult, and from $1\frac{5}{8}$ to $1\frac{3}{4}$ inch in the child. Its anterior border reaches forwards as far as a vertical falling slightly behind the position of the mid-transverse tape. Its width (Photographs 37, 38, 39, and 40) is $4\frac{1}{4}$ inches in the adult, and 3 inches in the child.

The relations of the tapes to the median surface of the hemisphere (Photographs 24, 25, and 26) are practically identical. The circumferential one passes over the lower part of the marginal and the hippocampal gyri, over the lingual lobule, and comes into relation with the posterior extremity of the calcarine fissure.

In the fourth lateral space are found, not only the upper part of the cerebellum, but also the cuneus, portions of the lingual and quadrate lobules, and the gyrus fornicatus as it passes downwards to become continuous with the uncinate convolution, and, in addition, the parieto-occipital fissure.

In the third lateral and median spaces are seen the remaining portion of the quadrate lobule, a part of the gyrus fornicatus, the

paracentral lobule, the beginning of the marginal convolution, and the calloso-marginal sulcus.

The first and second lateral and the second median spaces contain the remaining portions of the marginal and fornicate gyri, and the calloso-marginal sulcus, while the relations of the mid-section of the corpus callosum to the transverse tapes are seen in the three photographs; in the adult it occupies portions of the four lateral spaces, but more of the first than the fourth lateral one. These relations are alike in all the adult heads, and in the older children; but in the one of four months (Photograph 26) the corpus callosum fails to reach the fourth lateral space.

The parieto-occipital fissure is also slightly nearer the posterior transverse tape in this child than in the adult heads. These are the only points of difference in the proportional relations which I have found to exist in the young and in the adult, and these are but slight.

The relations of the inter and mid brain with the pons and medulla are exhibited in these photographs, and the particular inch, or half-inch, at which they may be struck from the external surface can be read from the tapes; and their relations from the top in Photograph 30 will be found to be proportional, as was the case with the several divisions of the cortical surface.

Again, the relations of the islands, the inter, mid, and hind brain with the medulla are illustrated in Photographs 38, 39, 40, and 41, from the back, and follow the same rule.

In addition to the relations of the external and central parts of the brain to the scalp, these views give accurate guides to the surgeon to reach every cranial and spinal nerve shown in them. He has only to glance at Photographs 25, 26, 38, 39, 40, and 41 to obtain the position of any of the cervical or upper dorsal nerve-roots to the tape measurements, in the adult or in the child, from the root of the nose; the median longitudinal tape in all the back views being prolonged to the lower part of the neck if adult, or to the lumbar region in children. In glancing at the photographs he can easily detect the small first cervical, then the others can be read off in their order as low as the third dorsal in the

case of the adult, or to the lower lumbar in the case of the children.

In this short description of the relations of the deeper lying structures to the scalp surface I have purposely refrained from entering into wordy detail. To the educated eye these photographs require little, if any, explanation.

SUMMARY AND GENERAL RULES.

Were I a surgeon, called upon to open the living head at any age after birth, I should proceed as follows:—

If the seat of brain lesion is situated on the lateral aspect of the external surface of the brain—that is, below the line of the temporal ridge, I would depend on Photographs 14 to 19; if it is between the mid-sagittal line of the head and the temporal ridge externally, I would select Photographs 28, 29, and 31; if it is directly underneath the ridge itself, I would depend on Photograph 27; if it is situated behind, I would select Photographs 32, 35, 37, or 38; and if it is in front, I would turn to Photograph 42, or the anterior portion of the side or top series of photographs. After locating the area to be operated on to the median or lateral longitudinal tapes, or to the circumferential one, by a glance at any of the above-mentioned photographs, then, as the circumference of the illustrated head is to the circumference of the living one, so is the position of the area on the above tapes in the illustrated head to the desired position of the same area in the living one.

As I have illustrated so many different heads with the circumference varying from 20¼ to 23½ inches (after shaving the scalp), a maximum difference of 3¼ inches, and as the average adult head to be operated on will not vary more, a glance at the photograph illustrating the head with a circumference nearest the living one will be all that is necessary for the surgeon; so that the rule frequently mentioned, although it is well to know it, may almost remain a dead letter.

Should the surgeon desire to reach any of the central structures he will select the corresponding photographs—those showing the

relations of the central lobes and ventricle, or the others illustrating the relations of the inter, mid, hind, and after brain, the position of these following practically the same law as in the case of the cortex.

It is unnecessary that the surgeon should apply all the tapes to the scalp which appear on the photographs; the median longitudinal or the circumferential will suffice, as the position of any area can be readily enough found on verticals from the latter, or on lines parallel to the mid-transverse tape on the former.

It would not be difficult for an instrument-maker to design an apparatus that could be fitted on the vault of the head in the position of the tapes, and with corresponding measurements, somewhat similar to Wilson's cyrtometer, or other contrivances at present in use for locating the position of the central, or Rolando's, sulcus.

DESCRIPTION OF THE PLATE.

The plate is a reduction from the life-size composite (Photograph 14) referred to in the text, and Plate 14 of my book. It shows the relations of the cortical surface to the inch measurements. The Arabic numerals which appear on the plate are placed on the well-known sub-divisions of the various lobes of the brain. The numbers 1, 2, and 3 are also placed in the fissure of Sylvius, the sulcus of Rolando, and the parieto-occipital fissure respectively.

DR. ALEC. FRASER

ON OPERATIONS ON THE BRAIN.

THE INTRAPARIETAL SULCUS OF THE BRAIN.

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[Read in the Section of Anatomy and Physiology, January 3, 1890.]

The intraparietal sulcus of the brain was first described and named by Sir William Turner, in 1866, in a paper on the Brain of the Chimpanzee.[a] In the month following he described it in the human brain, in a lecture[b] which he delivered to the Royal Medical Society in Edinburgh. Prior to this it had, however, been accurately figured, not only in the brain of man, but also in the brain of the ape. The description which Sir William Turner gave has in all its essential features been followed in most of our anatomical text-books. According to Professor Schäfer[c] the sulcus "ascends at first parallel to the furrow of Rolando and then turns backwards horizontally to the back of the lobe, extending nearly to the termination of the parieto-occipital fissure, past which it is often continued into the occipital lobe."

It therefore consists of a vertical part, which separates the supramarginal convolution from the ascending parietal convolution, and a horizontal part, which intervenes between the superior and inferior parietal lobules. But, in addition to these, a furrow is mentioned, under the name of the post-central sulcus, which stands in close association with the intraparietal sulcus. It is placed above the vertical part of the latter, and is interposed between the ascending parietal convolution and the superior parietal lobule.

[a] Proc. Roy. Soc., Edin., 19th Feb., 1866.

[b] "The Convolutions of the Human Cerebrum topographically considered." Edinburgh Medical Journal, June, 1866, and as a separate publication, Edinburgh, 1866. In the same year the intraparietal sulcus was also independently described by Dr. Adolf Pansch, of Kiel, under the name of sulcus parietalis (De Sulcis et Gyris in Cerebris Simiarum et Hominum, Kiliae, 1866).

[c] Quain's Anatomy. 9th Ed., Vol. II., p. 336.

Leaving out of count the relations which the intraparietal sulcus establishes in the occipital lobe, the above description is all that could be desired from a morphological point of view. It recognises the triple constitution of the parietal portion of the sulcus, and hints at occipital connections which are of the highest interest and importance. It fails, however, in so far that the arrangement of the three limbs which is detailed is not the usual one. There are several varieties of intraparietal sulcus. The one which is referred to in the above description only occurs in about 16 per cent. of cerebral hemispheres. All the different varieties of the sulcus can be explained and understood by a reference to its mode of appearance in the fœtal brain, and to its condition in the brain of the ape. In what follows we shall distinguish the three parietal limbs of the sulcus under the names of (*a*) *ramus verticalis inferior*, (*b*) *ramus verticalis superior*, and (*c*) *ramus horizontalis*. The occipital part we shall designate *the ramus occipitalis*.

The development of the intraparietal sulcus is attended with so much variability in different brains that it is a matter of extreme difficulty to decide upon what is in reality the typical arrangement of its several parts. The details which I give under this head are derived, not only from an examination of fœtal brains in my own possession, but also from a study of the masterly memoirs of Ecker[a] and Pansch,[b] and of the plates which accompany the writings of Reichert[c] and Bischoff.[d]

The sulcus in the sixth month of intra-uterine life is in the form of two parts—viz., an anterior and a posterior. The anterior part appears in the interval between the horizontal limb of the Sylvian fissure and the fissure of Rolando. It ascends for a short distance and then curves backwards over the extremity of the Sylvian

[a] Ecker, "Zur Entwicklungsgeschichte der Furchen und Windungen der Grosshirn-Hemisphären im Fœtus des Menschen," Archiv für Anthrop. dritte Band. drittes und viertes Heft, 1869.

[b] Pansch, "Ueber die typische Anordnung der Furchen und Windungen auf den Grosshirn-Hemisphären des Menschen und der Affen," Archiv für Anthrop., 1869.

[c] Reichert, Der Bau des Menschlichen Gehirns, Leipzig, 1861.

[d] Bischoff, "Die Grosshirn-windungen des Menschen," Aus den abhandlung der k. bayer Akad. der Wiss., 11 cl. x. bd., 11 Abth.

fissure (*vide* Reichert, Pl. XII., Figs. 49 and 50; also Bischoff, Pl. IV., Figs. 7, 8, 9). It may be regarded as representing the ramus verticalis inferior and the ramus horizontalis of the fully developed sulcus. The hinder part is the *ramus occipitalis*, and it usually appears as a distinct factor. It extends backwards on the outer face of the hemisphere a short distance from its upper margin. Commencing in front of the notch formed by the upper end of the internal perpendicular fissure it proceeds backwards beyond it into the occipital lobe (*vide* Ecker, Pl. II., Figs. 1, 6, and 7; also Reichert, Pl. XII., Figs. 50 and 51). Reichert reckons it as one of his "peripherischen Primärfurchen." But an additional part of the same system of furrows soon makes itself evident. This is a short vertical sulcus behind the upper part of the fissure of Rolando (ramus verticalis superior). Reichert also includes it amongst the "peripherischen Primärfurchen." It may remain distinct and separate (Reichert, Pl. XII., Figs. 49, 50, and 51; also Bischoff, Pl. IV., Fig. 9), more frequently it extends downwards and joins the ramus verticalis inferior (*vide* Ecker, Pl. II., Fig. 7). By this union a long furrow is formed behind the fissure of Rolando, and it is to this that Ecker gives the name of *sulcus postcentralis.*

In the admirable account which is given by Pansch of the typical arrangement of the furrows on the fœtal hemisphere, the *ramus occipitalis* of the intraparietal sulcus is not enumerated as a "Primärfurche." In other respects, although expressed differently, his description accords with the foregoing. He remarks—"Also auch hier, wenn denn doch einmal ein sogennanter allgemeiner Typus aufgestellt werden soll, wird man einen 'Stamm,' einen 'obern Ast,' und einen 'hintern Ast' der dritten radiären Primärfurche annehmen dürfen." The third radial primary furrow to which he refers is the intraparietal sulcus—the stem is the *ramus verticalis inferior*, the upper branch is the *ramus verticalis superior*, and the hinder branch is the *ramus horizontalis.*

But, as I have mentioned, considerable variability is exhibited in the mode of appearance and in the relation to each other of the different parts of the intraparietal sulcus, and such being the case, we can only appreciate the morphological importance of its different

factors by an appeal to the brain of the ape. By this means we are able to accord to each its proper value. Let us take the cerebrum of a baboon. I select this ape, not on account of its presenting an arrangement of the convolutions which may be regarded as being more typical than that found in many other species, but because I happen to possess fourteen cerebral hemispheres taken from the Chacma baboon, and the occasional variations which one meets in the disposition of the furrows in the same species are often of considerable value in the determination of homological relationships.

In the baboon, as in the vast majority of apes, the most apparent part of the intraparietal sulcus is present in the form of a sharply-cut oblique fissure, which traverses the parietal lobe from its antero-inferior angle to its postero-superior angle (Plate, Fig. 5, *c*). This fissure represents the ramus verticalis inferior, the ramus horizontalis, and a portion of the ramus occipitalis of the human sulcus. The ramus verticalis superior is in many apes entirely unrepresented; but in the baboon, and, indeed, in a large number of other forms, there is a more or less distinct indication of it present (Fig. 5, *e*). It may take the form of a stellate depression behind the upper part of the fissure of Rolando, or it may be present in the shape of a shallow linear furrow. Both conditions are met with in the baboon, and in three of the fourteen cerebral hemispheres which I have before me this furrow extends downwards, and effects a junction with the main part of the fissure, thereby indicating a subdivision of the latter into two portions corresponding to the lower vertical and horizontal parts of the human sulcus. This appears to be the usual condition in the chimpanzee (Fig. 2) and the gibbon. I found it present in three specimens of the former and in one of the latter. Bischoff also figures it in the gibbon,[a] and several authors in the chimpanzee. In the orang, however, the ramus verticalis superior remains separate, whilst in the gorilla it may present either condition.[b]

[a] Beiträge zur Anatomie des Hylobates leuciscus, pl. ii., fig. 1; aus Abhandlung der K. bayer. Akhad. der W., Cl. xi. Bd. x. Abth. iii., 1870.

[b] Broca, Mémoires sur le Cerveau de l'homme, publiés par le Docteur S. Pozzi, p. 636.

The *ramus occipitalis* of the intraparietal sulcus as it is seen in the ape presents some features of extreme interest; but it is better to defer these until we deal with the corresponding segment of the fissure in the human brain.

Putting aside then for the present the ramus occipitalis, it is clear, both from the study of the fœtal brain and the brain of the ape, that of the remaining three parts of the intraparietal sulcus, two, viz., the ramus horizontalis and ramus verticalis inferior, are originally continuous and identical, whereas the third, the ramus verticalis superior, may be looked upon as having an independent origin. I have recently examined sixty-two human cerebral hemispheres with the view of ascertaining the more usual disposition of the intraparietal sulcus. Amongst these I met with every possible form of combination of the three segments of the sulcus. No less than five varieties may be recognised.

Variety I. All the three parts of the sulcus separate.—It is very uncommon to meet with a cerebral hemisphere in which all the three segments of the intraparietal sulcus are separated from each other by superficial and distinct bridging convolutions.

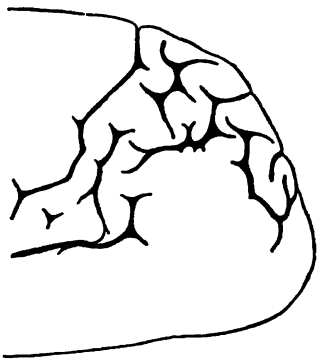


Fig. 6.—Posterior Part of the Left Hemisphere of a young man 25 years old. The four factors of the intraparietal sulcus are all separate and distinct.

Of the sixty-two hemispheres examined, only four exhibited this condition. These comprised (*a*) both hemispheres of a newly-born full-time male child; (*b*) the right hemisphere of a boy of 5 years old; (*c*) the left hemisphere of a young man 25 years old.

Variety II. Ramus horizontalis confluent with the ramus verticalis inferior ; ramus verticalis superior separate.—This is the condition of the intraparietal sulcus which was originally described by Sir William Turner, and in the last edition of Quain's Anatomy it is given as the normal arrangement. The development of the fissure and its condition in the ape would both alike appear to indicate this variety as the typical one, but certainly it is not the most

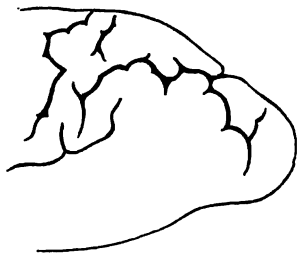


FIG. 7.—Left Hemisphere of a new-born Male Child. It shows the typical condition of the intraparietal sulcus.

common. It was only discovered in ten out of the sixty-two hemispheres examined, or, in other words, in 16 per cent. The hemispheres in which it occurred were derived from the following sources:—(*a*) Two from a newly-born full-time male child; (*b*) one from a girl aged 4 years; (*c*) one from a boy aged 5 years; (*d*) one from a youth aged 15 years; (*e*) two from two adult male negroes; (*f*) three from three adult Irish males.

In three of these the ramus horizontalis was partially separated from the ramus verticalis inferior by a deeply-placed annectant gyrus which passed from the superior parietal lobule to the supramarginal convolution. Sir William Turner speaks of the angle of junction of the ascending and horizontal parts of the sulcus as a frequent seat for a bridging gyrus in the human brain. It indicates a tendency for the horizontal limb to become divorced from the lower vertical limb, and affords the explanation as to the manner in which Variety I. of the sulcus is produced. The presence of this bridging gyrus is peculiarly a human characteristic. So far as I am aware, it has not been noticed in any ape

brain, and I have failed to discover it in the specimens of the brains of the chimpanzee and orang, which I possess.[a]

Variety III. Vertical limbs confluent; horizontal limb separate.—In the Plate, Fig. 1, a well-marked example of this variety of the fissure is represented. At first sight the hemisphere appears to be traversed by two fissures of Rolando and three central convolutions. The hinder of the two transverse sulci is the result of the complete union of the two vertical limbs of the intraparietal sulcus. It is the *sulcus post-centralis* of Ecker or the *sulcus post-rolandicus* of Pansch divorced from the ramus horizontalis. A thick superficial bridging convolution which connects the superior parietal lobule and the supra-marginal convolution cuts off the ramus horizontalis completely. No evidence as to its double nature can be gained by examining the bottom of this long transverse furrow. It is uniformly deep throughout, and possesses a depth not far short of that of the fissure of Rolando. Its walls interlock through the presence of numerous secondary gyri, but there is no trace of a bridging convolution.[b]

It is not common, however, to find a specimen of this variety of the intraparietal sulcus so characteristic as that figured in the Plate. I possess only one other hemisphere, obtained from an adult male, in which all the essential features are the same. Nevertheless, of the sixty-two hemispheres examined, ten came within the same class. In all of these there was a long continuous furrow placed behind the ascending parietal convolution, and parallel with the fissure of Rolando. As a rule, the upper part of this sulcus was shallower than the lower part, and in three cases it was partially cut off from it by a deeply-placed bridging gyrus, which crossed the bottom of the furrow and connected the poste-

[a] As we shall see later on, the occipital ramus of the intraparietal sulcus is, in the great majority of cases, also separated from the main portion of the sulcus by a superficial or deep bridging convolution. These links between the upper and lower parietal lobules are regarded by Wernicke as typical in the human brain (Arch. f. Psychiatrie, Bd. vi. S. 323). In the baboon a concealed gyrus sometimes marks off the occipital part of the sulcus.

[b] A condition of the intraparietal sulcus similar to this was described by Luys, "Description d'une circonvolution supplémentaire signalée dans certains cerveaux humaine," Gaz. d. hôp., Paris, 1876, xlix. p. 588.

rior parietal lobule with the ascending parietal convolution. Further, the superficial bridge which intervened between the horizontal ramus of the fissure and the vertical part was not so strongly marked. In the memoir by Ecker upon the development of the furrows and convolutions of the human brain, this condition of the intraparietal sulcus is represented in the process of formation in the right hemisphere of a seven months' fœtus (Plate II., Fig. 6); and in the atlas accompanying Gratiolet's *Mémoire sur les plis cérébraux de l'homme* the same variety is exhibited in a very complete form in the left hemisphere of the well-known "Hottentot Venus."

The ten hemispheres which I have described as belonging to this variety were taken from the following subjects:—(*a*) One from a girl, aged four years; (*b*) two from two adult negro males; (*c*) six from Irish adult males; (*d*) one from an Irish adult female.

Variety IV. The three parts of the sulcus confluent.—This is by far the most usual condition of the intraparietal sulcus in the human brain. It was present in thirty-five of the cerebral hemispheres examined, or, in other words, in about 56 per cent. The

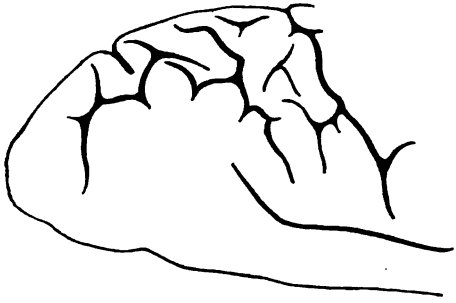


FIG. 8.—Posterior Part of the Left Hemisphere of an elderly Female. The more usual condition of the intraparietal sulcus is exhibited.

examples to which I refer were obtained as follows:—(*a*) Four from female children, aged respectively 6 months, 1 year, and 11 years; (*b*) two from two boys, aged 12 and 15 years; (*c*) fourteen from adult females (including two negresses); (*d*) eight from adult Irish males; (*e*) seven from Irish adults, whose sex could not be ascertained.

In several of these indications were present which pointed to a tendency on the part of the sulcus to approximate the conditions exhibited in Varieties II. and III. Thus, in six a deep concealed bridging gyrus passed between the superior parietal lobule and the supra-marginal convolution, and partially cut off the horizontal limb of the sulcus. Féré[a] has stated that in 75 per cent. this annectant gyrus is present. Certainly this is not my experience.

On the Continent it is usual to describe this variety of the intraparietal sulcus as the normal arrangement;[b] and in the numerous memoirs in which the cerebral hemispheres have been drawn from nature, it will generally be observed to be the condition depicted. It is further interesting to note that the chimpanzee (Fig. 2), gibbon, and in some cases the baboon and other apes, exhibit a similar disposition of the three rami of the intraparietal sulcus.

In man, therefore, we may say that the general tendency exhibited is towards a union of the two originally distinct vertical limbs, and a divorce from those of the horizontal limb, which in its origin and history is clearly continuous with the lower vertical limb.

Variety V. Ramus horizontalis joined to the lower end of he ramus verticalis superior, lower vertical limb separate.—This is a very rare form of the intraparietal sulcus. It was noted in only two of the sixty-two hemispheres examined, viz., in the right hemisphere of a negress and in the left hemisphere of an adult subject obtained in the dissecting-room, and of which the sex was not ascertained. It is difficult to account for this variety. In several cases belonging to Class IV. I observed the long sulcus, formed by the union of the two vertical rami, crossed by two deep and concealed gyri. The upper of these was placed at what might be supposed to be the junction between the two vertical limbs, whereas the second bridged across the ramus verticalis inferior. If we were to suppose the latter to come to the surface and the former to remain undeveloped, the condition exhibited by this variety would be produced.

[a] I have not been able to obtain the article by Féré in which this statement is made. The quotation is taken from Schwalbe's Neurologie, p. 552.

[b] For example, see Schwalbe's work on Neurologie, p. 551, and Gegenbaur's Text-book of Anatomy, p. 842, vol. ii., 3rd edition.

Of the sixty-two cerebral hemispheres examined, we have been able to refer sixty-one to one or other of the five varieties indicated. In the hemisphere which could not be so disposed of there was an almost total absence of the ramus verticalis superior of the intraparietal sulcus. Immediately above the junction of the ramus horizontalis with the vertical stem, the latter ran across the ascending parietal convolution, and joined the fissure of Rolando. This condition was brought about by a bending down of a portion of the ascending parietal convolution, in the form of a deep-concealed gyrus. And whilst referring to this case, I may mention that a junction between the ramus verticalis superior and the fissure of Rolando is occasionally met with. As a rule, however, the whole of the limb involved is not absorbed, as in the instance referred to.

At a recent meeting of the Anatomical Society, Professor Lockwood exhibited the cast of a brain which presented an extremely interesting anomaly. His description runs thus:—

"The right fissure of Rolando was as follows:—It began above by opening into the great longitudinal fissure, and after descending downwards and forwards above an inch, bifurcated to enclose a long narrow convolution. The two limbs which embraced this convolution ended by opening separately into the posterior limb of the fissure of Sylvius." . . .

Mr. Lockwood was inclined to believe that the "anomalous convolution" belonged to the parietal lobe.[a] I have not had the advantage of examining this interesting cast, but I believe that the condition is produced by the ramus verticalis inferior of the intraparietal sulcus running across the ascending parietal convolution and joining the fissure of Rolando. The figure which accompanies the description seems to indicate this. The "anomalous convolution," therefore, would represent the lower two-thirds of the ascending parietal convolution.

I would not venture to offer this explanation had I not observed somewhat similar cases in my study of the different varieties of intraparietal sulcus. Both of the vertical limbs may establish connections with the fissure of Rolando. The upper limb may

[a] Jour. Anat. and Phys. Vol. XXIII., part III.; Proc. Anat. Soc., p. 17.

run bodily into the Rolandic fissure, whilst the lower limb may be connected with it by a secondary branch. In all I have seen four cases of this kind, but it is not necessary to describe these separately or in detail. In three the upper vertical limb of the intraparietal sulcus alone was involved. It joined the fissure of Rolando at a varying distance from the upper free margin of the hemisphere. In one case, already referred to, it was almost entirely absorbed in this way. In the fourth case there was a double connection. Not only did the upper vertical limb run into the fissure of Rolando about an inch distant from the upper margin of the hemisphere, but also the lower vertical limb was joined to it by a cross-branch. In every instance the junction was effected by a knuckling down of the ascending parietal convolution, and this could easily be seen by opening up the lips of the junctional sulcus. When such a connection occurs, it appears to be common

Fig. 9.—Posterior Part of Right Hemisphere of a Female 35 years old. It shows the ramus verticalis superior crossing the ascending parietal convolution and joining the fissure of Rolando.

for the ascending frontal convolution to be divided in one or more places as well. This occurred in three of my cases. In these the upper part of the precentral sulcus effected a communication with the fissure of Rolando, and in Mr. Lockwood's figure a similar condition appears to exist.[a]

[a] Since the above was written, Mr. Lockwood has had the kindness to send me the cast of the brain in question. The arrangement of the sulci is more complicated than I had anticipated; but I still believe that the explanation which I have given above holds good. The three parietal parts of the intra-

We know the influence which the shape of the cranial envelope has upon the direction of the sulci of the brain. A restraint placed upon the growth-energy of the cerebral surface in a longitudinal direction tends to produce sulci and convolutions, which pursue a course at right angles to the direction of the restraint. On the other hand, a restriction placed upon the surface-growth in the vertical direction predisposes to the formation of oblique or longitudinal sulci.[a] A dipping down of one or other of the central convolutions, as in the cases referred to, may naturally then be ascribed to the latter cause.[b]

Ramus Occipitalis of the Intraparietal Sulcus.—This is undoubtedly the most interesting part of the intraparietal sulcus, and in the human brain its exact homology, with the condition present in the ape brain, is somewhat difficult to ascertain. A careful description of its course and relationships is given by Schwalbe,[c] Ecker,[d] Gegenbaur,[e] Pansch,[f] and other authors. Schwalbe states that after overstepping the artificial boundary line of the occipital lobe, the ramus occipitalis may end by joining the sulcus occipitalis transversus of Ecker, or be continued to the occipital pole as the superior occipital sulcus. The transverse occipital sulcus is a short

parietal sulcus are all separate, and the ramus verticalis inferior cuts the ascending parietal convolution in two, and thus effects a junction with the fissure of Rolando. It affords an example then of the rare Variety I.

[a] "Ueber den Einfluss der Schädelform auf die Richtung der Grosshirnwindungen," Von Ludwig Meyer, Centralblatt für die Med. Wiss. 1876, Oct. 1, No. 43; also article by Meynert, "Medizinische Jahrbücher von der K. K. Gesellschaft der Artze," Jahrgang 1876.

[b] I am aware that this contention would be disputed by Richter, who puts so much faith in the "pulsatorische Erschutterungen" of the hemispheres for the production of the secondary furrows ("Ueber die Entstehung der Grosshirnwindungen," Archiv. für path. Anatomie, Virchow, 1887, p. 398). The late Professor Pansch would also in all probability have taken exception to it. In his article. entitled "Bemerkungen über die Faltungen des Grosshirns," &c. (Archiv f. Psychiatrie, Band viii., Heft 2), he remarks, "Den Meynert'schen Anschauungen uber die Abhängigkeit der Windungsrichtung von der Form des Schädels vermag ich vorläufig nicht beizustimmen."

[c] Lehrbuch der Neurologie.

[d] "Entwicklungsgeschichte der Furchen und Windungen," &c., &c., already referred to; and also his book upon "The Human Brain."

[e] Lehrbuch der Anatomie des Menschen, Band 11.

[f] Die Furchen und Wulste, Berlin, 1879.

furrow which runs transversely across a small extent of the outer surface of the occipital lobe a short distance behind the external parieto-occipital fissure. It is very generally regarded as the representative of the so-called external parieto-occipital fissure in the ape, and therefore as a distinct and independent fissural integer. This conception of the sulcus transversus we believe requires some slight modification. Further, the account given by Schwalbe is apt to produce the impression that the *ramus occipitalis* is always continuous with the main portion of the intraparietal sulcus, whereas this is very far from being the case.

In no less than thirty-six of the sixty-two hemispheres which I have examined the ramus occipitalis was quite distinct and separate from the horizontal limb of the intraparietal fissure. Further, in eleven of the twenty-six cases in which it was confluent with the intraparietal sulcus a deep bridging convolution was found partially interrupting the course of the furrow at the point of junction. In those cases where the ramus occipitalis was separate, it began, as a rule, in the superior parietal lobule above the extremity of the ramus horizontalis of the intraparietal sulcus.

A point worth noting is the fact that of sixteen cerebral hemispheres obtained from children, twelve presented the ramus occipitalis in a separate form. With regard to adult brains an equal proportion of both sexes was found in each class, and the same may be said for the negro brains which were examined.

Upon phylogenetic grounds I have been led to believe that the sulcus occipitalis transversus of Ecker cannot be regarded as entirely corresponding with the so-called external parieto-occipitalis fissure in the ape; nor can it be regarded as constituting an entirely independent fissural element. It belongs in part, if not in whole, to the same fissural system as the intraparietal sulcus. In support of this contention, it will be necessary to bring forward some facts in connection with the condition in which the sulcus is found in certain of the apes.

Let us, in the first instance, examine the simple arrangement which is present in the brain of *Cebus capucinus* (Fig. 4). The intraparietal sulcus ascends obliquely in an upward and backward

direction, and approaches the upper margin of the hemisphere where the internal perpendicular fissure makes its appearance on the outer surface. Here it bends upon itself and turns sharply downwards for a short distance under cover of the operculum.[a] The internal perpendicular fissure opens freely into the angle of bending. Three parts then are here recognisable, viz., a long ascending limb, a short descending limb, and the internal perpendicular fissure as it appears on the outer surface, and all three are directly and uninterruptedly continuous. The descending limb represents a part, but not the whole, of the ramus occipitalis of the intraparietal sulcus in man.

The arrangement which we find in the cebus may be regarded "as the ground-plan in most of the apes" (Pansch). But before we go further it is very essential that we should come to some understanding as to what is meant by the term "external parieto-occipital sulcus" as applied to the brain of the ape. Does it refer to the sulcus which is seen on the outer surface of the cerebrum, in a line with the upper end of the internal perpendicular fissure and along the anterior lip of the operculum, or does it refer to the bottom of the pocket enclosed by the operculum? If we give to the term the former signification, then we may state that such a sulcus does not exist in the human brain, for the simple reason that in it there is no operculum. If, on the other hand, we confine its meaning to the bottom of the slit and regard the operculum as a structure formed by the forward growth of the posterior lip of an original transverse furrow, it remains for us to see what relationship is presented to this by the sulcus transversus of the human brain.

Pansch has very aptly compared the occipital operculum of the ape to the fronto-parietal operculum which covers the island of Reil. In this case we may compare the bottom of the opercular recess in the occipital lobe to the furrow which bounds the island of Reil above, and the superficial furrow along the anterior edge of

[a] I possess three cerebral hemispheres taken from the cebus. In one of these the short descending limb ends before it reaches the anterior lip of the operculum.

the occipital operculum with the fissure of Sylvius. We know that the island of Reil is produced by a stoppage of outward growth over the area which it is ultimately to occupy. In the first instance there is no furrow in relation to its upper border; this only appears with the downward growth of the fronto-parietal operculum. The question which arises, therefore, is whether, seeing we have no occipital operculum in the human brain, it is likely that we should have a furrow corresponding to the "Affenspalte." I merely mention this as a difficulty in the way of our regarding the sulcus transversus as homologous with the bottom of the "Affenspalte"[a] Knowing so little, however, as we do concerning the mode of development of the occipital operculum, no great stress can be placed upon this argument.

In carrying out our comparison between the intraparietal sulcus in man, and the corresponding sulcus in the ape, it is necessary to remove or throw back the operculum.

We have already examined the intraparietal fissure in the cebus, and seen it end in the form of a short descending branch under cover of the operculum. This is quite distinct from the bottom of the opercular recess, which is carried backwards for a short distance behind it.

The baboon will serve for our next stage (Fig. 5). In the brain of this form there is a deeply-placed annectant gyrus (the first bridging convolution) which partially cuts off the internal perpendicular fissure from the intraparietal sulcus (*h*). This gyrus is sharply defined in front by a branch of the intraparietal sulcus which extends upwards in front of it (*f*). Posteriorly it is limited by a furrow which corresponds with the upper part of the bottom of the opercular slit (*i*), whilst externally it is limited by the short descending limb of the intraparietal fissure. The latter, however, has no connection with the bottom of the recess formed by the operculum.

The condition of the fissure in the chimpanzee (Fig. 2) leads us directly up to man. The first annectant gyrus is still partially

[a] The term "Affenspalte" is used in Germany to indicate the recess or slit under the operculum of the occipital lobe.

the ramus occipitalis, and breaks it up into two parts, which are separated from it by vertical annectant gyri. I have two brains in my possession which exhibit this condition, and in both the hinder bifurcation of the ramus occipitalis (sulcus transversus) is seen behind the fissure in question.

I have tried to show that the phylogenetic evolution of the sulcus transversus would seem to indicate that it is not altogether an independent sulcus, but belongs partly to the same system as the intraparietal. The ontogenetic development of the sulcus does not furnish evidence of a satisfactory nature. Ecker figures the brain of an eighth month fœtus in which the sulcus transversus is divorced from the occipital part of the intraparietal fissure, although in the same plate he depicts another brain of the same age in which the two sulci are continuous. Again, in Plate XVI. of the Atlas by Leuret and Gratiolet, a figure is given of the brain of a seven months' fœtus, in which the sulcus transversus is shown in direct connection with the ramus occipitalis of the intraparietal sulcus.

Pansch refuses to admit that the sulcus transversus is a primary sulcus.

Bischoff, it is true, describes and figures a sulcus under the name of the fissura perpendicularis externa. He includes it in the group of "Primärfurchen," and states that it appears in the seventh month, but vanishes in the eighth month of fœtal life. It belongs to that class of formations, therefore, he remarks, which only in certain forms attain their complete development, whilst in others they are arrested or completely disappear. This statement by Bischoff has never received confirmation. Ecker points out that most probably the fissure referred to is one which appears in the fifth month, and vanishes in the seventh month of intra-uterine life. It is one which I have studied closely in a large series of fœtal brains, and the relationships of which I purpose discussing in a memoir at present in course of preparation.[a] Although somewhat outside the scope of the present paper, I may briefly state some facts regarding it. On the outer face of the occipital lobe of the

[a] Cunningham Memoir No. VI., Royal Irish Academy, "A Contribution to the Anatomy of the Cerebral Hemispheres."

young embryo two fissures may make their appearance. These correspond in every respect with the internal perpendicular and calcarine fissures on the inner face of the hemisphere. The vertical infolding (Bischoff's fissura perpendicularis externa), according to my observations, very rarely occur. I have no specimens in my possession in which it is seen; but in a fœtal brain, preserved in the Oxford Museum, it is present in a well-marked form.[a] It is well figured by Bischoff, and has only a transitory existence. Bischoff is altogether wrong as to the time which he gives for its appearance and disappearance. With regard to the second fissure which appears on the outer face of the occipital lobe of the fœtus, the following facts may be noticed:—(1) It produces a distinct infolding of the hemisphere wall. (2) It is placed, not vertically, but very obliquely near the lower margin of the occipital part of the hemisphere. (3) It corresponds on the outer surface of the hemisphere to the calcarine fissure on the inner face, and the bulgings which they both form into the primitive ventricular cavity lie exactly opposite each other. (4) It apparently disappears in the human brain, but it is retained in the ape. (5) As the hinder end of the hemisphere becomes solid, and the ventricular cavity no longer reaches the extremity, it is only the anterior part of the calcarine fissure which forms the calcar avis; the fissure in question is shorter than the calcarine fissure, and in many apes its anterior end just falls short of the posterior horn of the ventricle; but in some it just reaches it, and in these cases it forms a bulging on the outer side of the ventricle corresponding to the calcar avis on the inner side.

I had not intended to discuss the homologies which undoubtedly exist between the fissures and convolutions of the carnivorous brain and those of the brain of the primates; but there are some aspects of the question which it is impossible for me to shirk. It is now very generally admitted that the anterior part of the uppermost arching furrow in the brain of the carnivora (the *sulcus coronalis*)

[a] It is through the kindness of Professor Victor Horsley that I have been able to make this observation. In fact, I may say that he saved me from falling into a serious error in connection with this question by furnishing me with a series of beautiful photographs of the specimens in the Oxford Museum.

has its homologue in the fissure of Rolando. Such being the case, the anterior limb of the second arching furrow (*sulcus suprasylvius*) is the representative of the intraparietal sulcus in the brain of man and the apes. So far everything is clear and satisfactory; but can we offer any explanation as to the homologies of the "Affenspalte," and the downwardly directed posterior part of the intraparietal sulcus in the ape (Plate, Figs. 4 and 5). In the brain of the cebus these sulci are quite distinct from each other, and in some cases the intraparietal sulcus ends altogether in front of the occipital operculum. In the present instance I can only deal very briefly with this question; but I may be allowed to state my belief that the descending limb of the intraparietal sulcus, so well exhibited in the brain of the cebus and other apes, is to be regarded as a part of the posterior limb of the middle arching fissure (*sulcus suprasylvius*) of the carnivorous brain divorced from its temporo-sphenoidal continuation (*parallel sulcus*). It is possible, therefore, that the "Affenspalte" may have its homologue in a portion of the posterior limb of the uppermost "Bogenfurche" of the carnivorous brain.

Sir William Turner, in his masterly article upon the cerebral convolutions of the seals and walrus,[a] has cleared away from our path one great difficulty in arriving at a true conception of the convolutions in the brain of the carnivora, by offering a most ingenious and highly probable explanation of the origin of the island of Reil in man and the apes, and of the disappearance coincident with this of the Sylvian convolution (lowest arching convolution) of the carnivorous brain.

[a] Jour. Anat. and Phys., vol. xxii.; also Zool. Chall. Exp. part lxvii., 1888.

Fig. 1

Fig. 2.

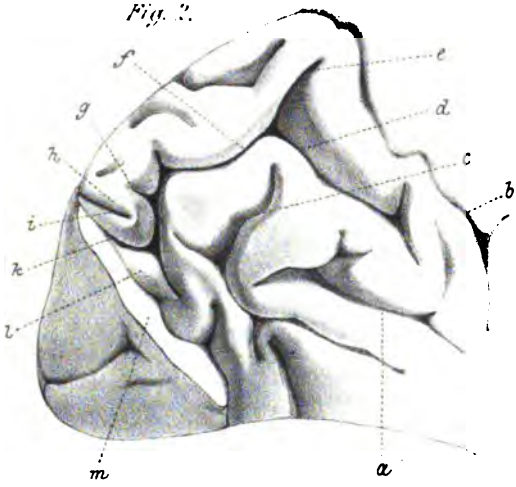


Fig. 3.

Fig. 4.

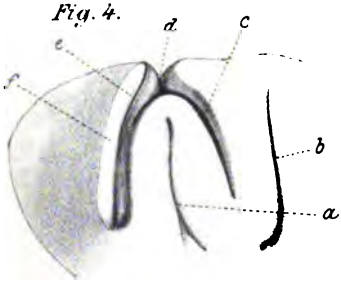


Fig 5.

INTRAPARIETAL SULCUS.

EXPLANATION OF PLATE.

Fig. 1. Right hemisphere of an adult male (drawn on the stone from a photograph). It exhibits, in a well-marked form, the condition described in the paper as Variety III. of the intraparietal sulcus. A, island of Reil; B, horizontal limb of the fissure of Sylvius; C, ascending limb of fissure of Sylvius; F, fissure of Rolando; G, external parieto-occipital fissure. *b* and *c*, second and third frontal convolutions; *d*, ascending frontal convolution; *e* and *h*, ascending parietal convolution; *f*, supramarginal convolution; *g*, angular gyrus; *i*, *l*, and *m*, occipital convolutions; *n*, *o*, *p*, temporo-sphenoidal convolutions; 1, 2, 3, and 4, annectant gyri.

Fig. 2. Posterior portion of the right hemisphere of a young female chimpanzee. *a*, fissure of Sylvius; *b*, fissure of Rolando; *c*, parallel sulcus; *d*, ramus verticalis inferior of the intraparietal sulcus; *e*, ramus verticalis superior of the intraparietal sulcus; *f*, ramus horizontalis of the intraparietal sulcus; *g*, the branch which limits the first annectant gyrus in front; *h*, the first annectant gyrus; *i*, the internal perpendicular fissure as it turns out on the upper surface of the hemisphere (external parieto-occipital); *k*, the branch of the intraparietal sulcus which turns up behind the first annectant gyrus to join the "Affenspalte;" *l*, second annectant gyrus; *m*, cut surface of the occipital operculum.

Fig. 3. The posterior part of the left hemisphere of the white crowned mangaby. *a*, parallel sulcus; *b*, fissure of Sylvius; *c*, intraparietal sulcus; *d*, ramus verticalis superior; *e*, descending limb of the intraparietal sulcus; *f*, upper end of internal perpendicular fissure turning outwards on the upper surface of the hemisphere (external parieto-occipital); *g*, cut surface of occipital operculum.

Fig. 4. Posterior portion of the right cerebral hemisphere of the *Cebus capucinus*, viewed from above. *a*, Sylvian and parallel fissures combined; *b*, fissure of Rolando; *c*, intraparietal sulcus; *d*, internal perpendicular fissure; *e*, "Affenspalte;" *f*, cut surface of occipital operculum.

Fig. 5. Posterior part of the right cerebral hemisphere of the Chacma baboon looked at from above. *a*, Sylvian fissure; *b*, parallel sulcus; *c*, ramus verticalis inferior of the intraparietal sulcus; *d*, fissure of Rolando; *e*, ramus verticalis superior; *f*, branch from intraparietal sulcus in front of the first annectant gyrus; *g*, the upper end of the internal perpendicular fissure; *h*, first annectant gyrus; *i*, "Affenspalte;" *k*, cut surface of the occipital operculum.

COLLECTIVE INVESTIGATION IN THE ANATOMICAL DEPARTMENT OF TRINITY COLLEGE, DUBLIN.

REPORTED BY W. H. THOMPSON, M.D., F.R.C.S. ENG.

[Read in the Section of Anatomy and Physiology, March 14, 1890.]

Formation of the Portal Vein. Investigated by Dr. W. H. Thompson.

IN issuing this as a subject for collective investigation, the Anatomical Society asked for information regarding the *three* great trunks which join to form the portal vein. But the investigator considered the matter would be imperfect were the coronary or superior gastric vein not also included, and so the results have proved.

The points noted were—(1) the order of union of the four great vessels; (2) the levels at which they joined; and (3) the presence or absence of valves in their tributaries.

(1) *The order of union of the four great trunks.*—Taking it as a fact that the splenic vein always unites with the superior mesenteric vein, there are then only two other veins whose terminations have to be dealt with—viz., the inferior mesenteric and coronary veins. Now, of these the inferior mesenteric was examined in 58 subjects (26 males and 32 females), and was found to end as follows:—

(*a*) In the splenic vein, in 33 subjects—19 males and 14 females;
(*b*) In the inferior mesenteric vein, in 22 subjects—18 females and 4 males;
(*c*) In the angle of union between these two veins, in 3 subjects, all of whom were males.

The coronary vein, however, was examined only in 48 subjects (32 males and 26 females), and was found to terminate—

(*a*) In the portal vein, in 29 subjects—15 males and 14 females;
(*b*) In the splenic vein, in 19 subjects—7 males and 12 females.

Thus the complete formation of the portal vein, so far as these four great trunks are concerned, was examined in 48 subjects. This brought out the result that there are four chief ways in which the vessel may be formed—

(1) That which is usually described as normal, where the inferior mesenteric vein enters the splenic vein, while the coronary, or superior gastric, enters the portal vein (Fig. 1). This condition existed in 18 out of the 48 subjects examined. Nine of these were males and nine females.

(2) That in which exactly the reverse condition obtains, where the inferior mesenteric vein joins the superior, while the coronary vein enters the splenic—where, in other words, the inferior mesenteric and coronary veins have, so to speak, changed places (regarding, for the moment, the superior mesenteric and portal veins as one continuous trunk) (Fig. 2). This condition occurred in 11 out of the 48 subjects examined, nine being females, while only two were males.

Fig. 1.

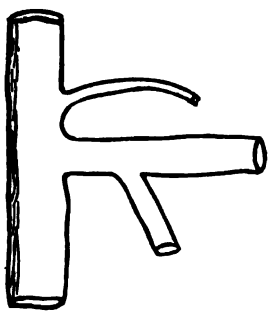
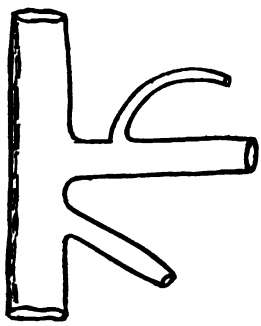


Fig. 2.



(3) That in which both inferior mesenteric and coronary veins pour their blood into the splenic vein (Fig. 3). In 8 out of the 48 subjects examined this was the mode of ending of these veins, 4 being males and 4 females.

(4.) That in which neither the inferior mesenteric nor coronary veins enter the splenic, but both of these, together with the splenic vein, successively join the continuous trunk formed by the superior

mesenteric and portal veins (Fig. 4). This group also included 8 cases—4 males and 4 females.

Fig. 3.

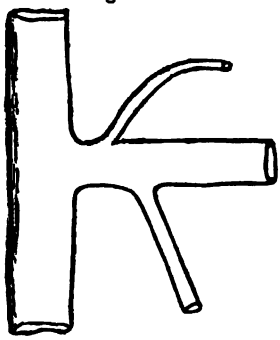
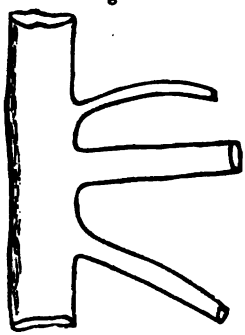


Fig. 4.



Those cases (3 in number) in which the inferior mesenteric vein joined exactly at the angle of union between the splenic and superior mesenteric veins were too few to make into a separate group. Most cases can be referred to one or other of the above groups. Though not inclined to lay much stress upon it, the investigator would like to point out the much greater relative frequency with which the second mode of formation occurred in females.

2. *The levels at which the Portal Vein was formed.*—This was observed in 47 subjects.

In 4 the level was that of the lower part of the first lumbar vertebra;

In 4 the disc between first and second lumbar vertebræ; in 33, opposite the second lumbar vertebra;

In 5 the disc between second and third lumbar vertebræ; and in 1 instance opposite the upper part of the third lumbar vertebra.

With regard to the exact distances between the vessels at their termination, it was found that the inferior mesenteric vein entered the splenic between one half and three-quarters of an inch from its termination in 19 cases; about one inch from its termination in 10 cases; a quarter of an inch or under in 4 cases, three of these

being exactly at the angle of union of the splenic and superior mesenteric veins.

In all cases the portal vein was formed behind the head of the pancreas.

3. *The presence of valves* was sought for in 12 subjects. In none of these were any found.

Abnormalities of the Renal Artery. Investigated by A. C. O'Sullivan, F.T.C.D., and O. L. Robinson, M.R.C.S.

The arterial supply to the kidneys was examined in 52 subjects. In one of these, however, the condition could only be noted on one side. 103 kidneys were therefore examined. As regards number, it was found that in 76 cases there was only one artery present; in 23 instances there were two; in 3 cases there were three; and in 1 instance there were four arteries present.

In the 23 instances where there were two arteries, the extra trunk was distributed as follows:—In 10 cases it entered the upper border of the kidney, the main trunk entering the hilum. In 5 it entered the lower border of the kidney, the main trunk also entering the hilum. In 4 of these the accessory trunk arose from the aorta below the main trunk, the remaining one arising from the right common iliac artery.

In regard to the relations of the artery or arteries to the vein and ureter at the hilum of the kidney, it was found convenient to divide them into six groups.

Group 1.—This was taken as the normal arrangement, in which the arterial trunk arising from the aorta passed outwards behind the vein, its branches entering the hilum between the vein and ureter. This condition existed in 52 out of the 103 kidneys examined.

Group 2.—Here the trunk of the artery also passed outwards behind the vein, but its branches enclosed the ureter at the hilum. This condition was observed in 21 instances.

Group 3.—The artery here also passed out behind the vein, but its terminal branches enclosed that vessel on entering the hilum. 12 instances of this variety were noted.

Group 4.—In this the artery, passing outwards behind the vein, broke up into branches which enclosed both the vein and ureter at the hilum. 8 cases of this variety were noted, in 5 of which there was also a branch passing between the vein and ureter.

Group 5.—In which the artery passed inwards wholly in front of the vein, its branches entering the hilum in the same position. Of this variety 8 cases were also noted.

Group 6.—In this the artery passed outwards behind the vein, breaking up into branches which entered the hilum altogether behind the ureter. Only 2 instances of this were noted.

The Relation of the Internal Maxillary Artery to the External Pterygoid Muscle. Investigated by J. J. Long, B.A. (Dub.).

Fifty-one subjects were examined, and in these the position of 93 arteries was noted—9 of the subjects being examined on one side only.

The artery lay superficial to the external pterygoid muscle in 47 of the 93 cases examined. This condition did not show any decided preference for one side more than the other—thus 22 were on the right side and 25 on the left.

In all these cases the artery passed into the spheno-maxillary fossa between the two heads of the external pterygoid muscle. The only other structure which bore any special relation to the artery was the long buccal nerve. This invariably came out between the heads of the muscle, in 13 cases appearing above the artery to pass down superficial to it, while in 31 cases it passed out below and deeper than the vessel.

The artery lay deeper than the external pterygoid muscle in 43 cases, and here it seemed to have a slight preference for the right side. Thus, of the 43 cases, 25 were on the right side and 18 were on the left.

When taking this position moreover it was found to have a very variable relation to the branches of the inferior maxillary nerve. This is shown as follows:—

(*a*) *The inferior dental nerve* in 19 instances passed down superficial to the artery, in 19 instances also it lay deeper than the vessel and in 5 it was perforated by it.

(*b*) *The lingual nerve* in 36 cases passed deeper than the artery, while in the remaining 7 cases it passed down superficial to it. In all these 7 cases the inferior dental nerve also passed down superficial to the artery.

There were two instances in which the artery pursued an intermediate course as regards the external pterygoid muscle by piercing its lower head of origin and then becoming superficial to it.

A few cases of special irregularities in the origin and course of the artery occurred to which it is only necessary to allude.

The Tuberculum Laterale of the Astragalus, or Os Trigonum. Investigated by C. E. Stokes, B.A. (Cantab. et Dub.).

One hundred and eight lower limbs were examined. Amongst these there were 3 well-marked examples of the os trigonum, while in a fourth case there were evidences which seemed to point to the original separate existence of the tubercle. In each of the 3 cases the ossicle articulated by means of a synovial joint with the main bone, the interior of the joint communicating with the posterior calcaneo-astragaloid articulation. In each of these cases also it gave attachment to a few fibres of the posterior fasciculus of the external lateral ligament of the ankle-joint.

The Superior Tibio-Fibular Articulation. Investigated by J. Barcroft Anderson, B.L.

This joint was examined with the object of determining in what proportion of cases a communication existed between it and the knee-joint. 100 legs were examined, and in 18 of these a communication between the two joints was found. In 3 of the others some anchylosis from disease existed.

The communication was invariably between the so-called bursal prolongation of the synovial sac which underlies the popliteal tendon on the external tuberosity of the tibia and the immediately subjacent synovial cavity of the tibia-fibular joint. The opening varied from a centimetre to a centimetre and a half in length.

THE PARIETO-OCCIPITAL AND CALCARINE FISSURES.

By D. J. CUNNINGHAM, M.D.;
Professor of Anatomy, University of Dublin.

[Read in the Section of Anatomy and Physiology, March 14, 1890.]

The views which have been advanced by different authors regarding the origin of the parieto-occipital and calcarine fissures are very conflicting, and no doubt there are many points in connection with their first appearance and history which are extremely puzzling.

v. Kölliker[a] maintains that they appear synchronously with the temporary fissures on the mesial aspect of the hemisphere, and constitute members of the same series—differing from them only in so far that they are permanent and not evanescent. He says:—"As early as the separation of the frontal lobe from the temporal lobe by the Sylvian fossa there arises a boundary for the occipital lobe by the appearance of the parieto-occipital fissure. This is distinct in the third month. Schmidt even figures it in the eighth week." Richter,[b] who gives a good account of the transitory furrows in a series of early embryos, appears to entertain the same view. Referring to a brain with hemispheres 3·3 c.m. long, he remarks:—"An der medialen Seite der Hemisphären gingen einige radiäre Falten mehr von der Bogenfurche aus als bei Fötus 3. Bei letzerem zählte man im Verlauf der ganzen Bogenfurche fünf, bei Fotus 4 neun. Die Fiss. parieto-occ. und calc. standen bei Fötus 4 zur Bogenfurche ungefähr noch in demselben Verhältniss wie bei Fötus 3. Ausser der Parieto-occ. und calc. verschwinden übrigen diese radiären Furchen der medialen Hemisphärenwände

[a] Entwicklungsgeschichte des Menschen und der höheren Thiere, 1879.

[b] "Ueber die Entstehung der Grosshirnwindungen," Virchow's Archiv., 1887, p. 398.

dieser Entwicklungsepoche sammt und sonders wieder und theilen so das Geschick der schon früher erwähnten occipitalen Falten früherer stadien, obschon sie wie diese Totalfalten im His'schen sinne sind."

Ecker[a] expresses himself on this question with considerable doubt and caution. "Moreover," he remarks, "it appears to me that one of the latter, viz., the fissura parieto-occipitalis is formed out of one of the temporary furrows, although I do not venture to assert this." This statement is rendered all the more ambiguous by the fact that in his description of the brain of a third-month fœtus he asserts that the occipital lobe does not exist. Undoubtedly the fissure in dispute (the precursor of the parieto-occipital) is present at this stage. Further, he figures the parieto-occipital and the calcarine fissures in a brain at the fourth month, although he appears to infer that they arise more frequently in the course of the fifth month.

According to Mihalkovics,[b] the calcarine fissure is the first to appear. It is formed at the end of the third month, and arises with the outgrowth of the occipital lobe almost simultaneously with the transitory furrows. The parieto-occipital sulcus is formed shortly after it at the commencement of the fourth month.

Having now stated the views of those authors who have dealt with this question, I shall proceed to state the conclusions at which I have arrived regarding the parieto-occipital and calcarine fissures in the course of my study of the complete fissures.

At the same time that the transitory fissures appear on the medial face of the hemisphere (towards the end of the second month, or it may be towards the beginning of the third month) two fissures which have a synchronous origin, and lie in series with these, occupy positions which give them a close resemblance to the parieto-occipital and calcarine fissures of the fully-developed brain. Between them is placed the cuneus. I have never seen these fissures absent, and in all good illustrations of the medial

[a] "Zur Entwicklungsgeschichte der Furchen und Windungen der Grosshirnhemisphären im Fœtus des Menschen," Archiv. für Anthrop., 1869.

[b] Entwicklungsgeschichte des Gehirns, Leipzig, 1876.

face of the hemisphere (v. Kölliker, Richter, &c.) they are represented. We have already referred to these infoldings as the precursors of the parieto occipital and calcarine fissures.

One or other, or perhaps in some cases both, of these precursors may be retained and ultimately form the respective adult fissure or fissures. Most frequently, however, one disappears whilst the other is preserved. That which is obliterated is replaced later on (in the fifth month, or towards the beginning of the sixth month) by the permanent furrow, and this takes up the same ground as its precursor, although it does not show an unbroken continuity of existence with it. It is questionable if both precursors ever disappear again to be replaced by secondary successors.

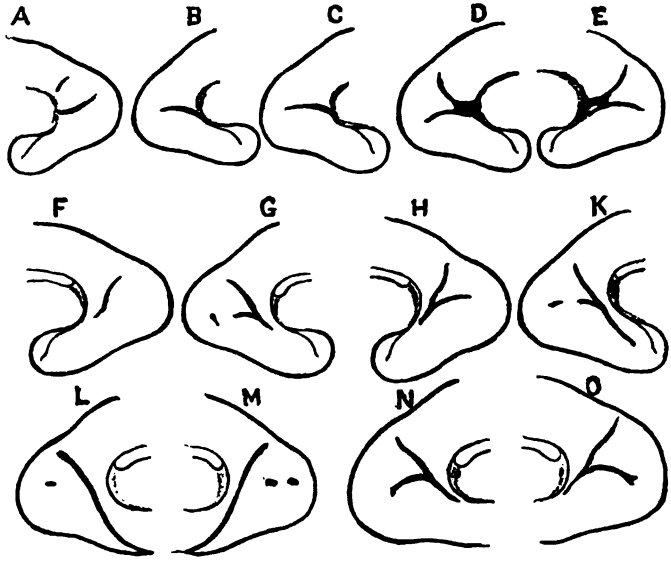


FIG. 7.—Different forms assumed by the parieto-occipital and calcarine fissures at different stages of their development.

It is difficult to say which of the precursory infoldings most frequently disappears. I am of opinion, however, that it is the parieto-occipital, and this view is quite in keeping with the phylogenetic evidence which we have on the question. In the apes

there cannot be a doubt but that the calcarine fissure is the most stable of the two. About the fifth month of intra-uterine life it is not uncommon to find hemispheres in which the parieto-occipital is totally absent, although the calcarine is present (Fig. 7, B and C). Sometimes, however, at this stage, in addition to a well-marked calcarine fissure, a faint trace of the parieto-occipital may be visible. In two hemispheres from the same fœtus, in my possession, the calcarine alone is present on the left side, whilst on the right side there is also a weak indication of the parieto-occipital (Fig. 9, A and B). It is hard to determine in this case whether the faintly-marked parieto-occipital is in process of obliteration, or in process of formation. On the ventricular face of the hemisphere wall a prominence corresponded with the weakly-marked parieto-occipital fissure, and this taken in conjunction with the fact that in the initial stages of the reappearance of the calcarine fissure, in cases where its precursor has been obliterated, I have failed to detect such a projection, makes me incline to the view that the parieto-occipital in this instance is really in process of obliteration.

His[a] has called attention to the fact that in certain fœtal brains of the fifth month the calcarine fissure may exist alone on the one hemisphere, whilst on the other the parieto-occipital only is developed.

But from the beginning of the fifth month, and onwards, another condition is frequently met with. In this both fissures are present (Fig. 7, D and E). They converge towards a broad shallow gutter or furrow, by which they are led forward into the hippocampal fissure. The typical ≺-shaped condition is thus assumed, and this persists throughout the further development of the brain. In such a case it is probable that we have a retention of both precursory fissures. His figures a hemisphere which shows this disposition of the fissures, and he considers that the brain from which the drawing is taken had reached a stage of development corresponding to four and a half months. From the fact, however, that the corpus callosum is represented in the fully-developed condition, and that traces of the calloso-marginal sulcus are indicated, it is

[a] Unsere Korperform, Neunter Brief, p. 114.

likely that the fœtus from which the brain was taken was somewhat more advanced.

I have only seen one hemisphere in which the parieto-occipital fissure was present alone, without a trace of the calcarine[a] (Fig, 7, F). This belonged to a fœtus which had reached a stage of development corresponding to the end of the fifth month. On the other hand, I have observed several in which the parieto-occipital was the predominant fissure, and in which the calcarine was so poorly developed that we could only conclude that it was in its initial stages of reappearance. It is somewhat curious that the hemispheres in which this condition was present were all at a somewhat advanced stage of development, viz., at the sixth month, or at least close upon it. In these cases the reappearing calcarine may assume several forms:—(1) It may appear as a short, weak branch, stretching backwards for a short distance from the lower part of the parieto-occipital (Fig. 7, H); (2) as a short branch in connection with the parieto-occipital, but also with an additional small detached piece lying nearer the occipital pole, and in the line of the future fissure (Fig. 7, G and K); (3) as one or two detached and isolated pieces which lie in the line of the future fissure, and which are in no way connected with each other or the parieto-occipital fissure (Fig. 7, L and M).

This last mode of development, viz., by isolated portions which ultimately run into each other, is frequently observed in connection with the cortical and permanent sulci. Thus the intra-parietal, the calloso-marginal, and the supero-frontal sulci frequently afford examples of this; and it is even not uncommon to see the same mode of development in the case of the fissure of Rolando.[b] When a section is made through the occipital lobe, in cases where the calcarine fissure is merely represented by these slight depressions, it is seen that they produce no corresponding elevation on the ventricular aspect of the hemisphere wall.

[a] Since writing, the above two additional hemispheres of this kind have come into my possession.

[b] The fact that the fissure of Rolando may first appear in two pieces—one near the free border and the other near the Sylvian fossa,—which ultimately run into each other, shows at once how erroneous the view advanced by Krause as to its orign is. To this we shall refer in a subsequent chapter of this memoir.

In both hemispheres of a fœtus which had arrived at the end of the fifth month, or the beginning of the sixth month, the parieto-occipital fissure presented a very remarkable development. Instead of ending a short distance below the hinder end of the corpus callosum, it was prolonged downwards and forwards as a deep fissure, which reached the lower border of the temporo-sphenoidal lobe (Fig. 7, L and M). The calcarine fissure was present as two short isolated portions or depressions, and the part of the parieto-occipital fissure below the line of these was at least three times as long as the part above. The long lower part must be regarded as transitory. In the further development of the brain it would, in all probability, have become obliterated. In this case the appearance presented by each hemisphere suggested as the cause of this long fissure the bending, in an inward direction, of the entire hinder part. The fissure formed the angle of bending.

The typical ≺-shaped condition of the combined parieto-occipital and calcarine fissures appears to be invariably assumed between the sixth and seventh months of fœtal life (Fig. 7, N and O), although, as we have seen, it is frequently assumed at the fifth month. Even at the seventh month, however, it is not at all uncommon to find a marked difference in the degree of development of the two fissures, and in the manner in which they influence the ventricular aspect of the cerebral wall in different brains. In some cases it is the calcarine which is deepest and continuous with the stem of the ≺. A distinct hippocampal elevation on the ventricular aspect of the hemisphere wall marks its course. The parieto-occipital fissure in such cases is a shallow furrow, and produced no ventricular eminence. In other instances exactly the reverse condition may be noticed. The parieto-occipital fissure is present as a deep infolding, continuous with the stem of the ≺, whilst the calcarine fissure is shallow. Consequently, on opening the posterior horn of the ventricular cavity by the removal of the outer wall, very different appearances may be seen in different brains:—(1) Two elevations, corresponding to the two fissures, may be seen on the ventricular aspect of the inner wall. I do not possess a specimen which shows this condition, but, where both

fissures persist from the precursory stage, it is reasonable to assume that the corresponding ventricular elevations will persist also. (2) In other instances, one ventricular elevation alone is present. This may correspond with the calcarine, in which case it proceeds in a more or less horizontal direction from behind forwards, or it may correspond with the parieto-occipital, in which case it descends from above, and takes a curved direction from above downwards and forwards. The deep fissure with the infolding on the ventricular aspect of the hemisphere walls may be regarded as the one which has persisted from the precursory stage. The other is secondary, having been, in the first instance, wiped out prior to its permanent reappearance.

Greatest Depth in Millimetres of the Parieto-occipital and Calcarine Fissures.

No. of Observation	Age of Fœtus	Parieto-occipital		Hinder part of Calcarine	
		Right Hemisphere	Left Hemisphere	Right Hemisphere	Left Hemisphere
1	Commencement of 7th month	2	**4**	3	**1**
26	28 weeks -	2	1	2	—
11	7th month -	**4**	**4**	**2**	**2**
38	29 weeks -	**3**	3	**5**	4
25	29 weeks -	5	4	5	**5**
15	30 weeks -	**6**	**7**	**3**	**2**

N.B.—Large figures are used where there is a striking difference in the depth of the calcarine and parieto-occipital fissures on the same hemisphere.

A study of an excellent paper which has recently been published by Dr. J. Mingazzini,[a] of Rome, makes it very evident that about the beginning of the seventh month it is extremely common to find one fissure deep and the other shallow. I may be allowed to

[a] "Ueber die Entwicklung der Furchen und Windungen des Menschlichen Gehirns," aus Untersuchungen zur Naturlehre des Menschen und der Thiere. Herausgegeben von Jac. Moleschott. XIII. Band, 6 Heft.

extract from the useful tables which this author gives the items contained in the Table on preceding page.

Pansch[a] raises a question as to the position which the stem of the ≺ occupies with reference to the two fissures. He says:—

"Zu welchen dieser beiden der Stamm des Y gehört, oder ob dieser wechselt, oder ob er weilleicht zuweilen isolirt ensteht, wage ich noch nicht endgültig zu enschieden."

This is an important question, because in the adult human brain it is the back part of the stem which is chiefly related to the calcar avis. The parieto-occipital fissure above the stem has no relation whatever to the ventricular cavity, because above the level of its junction with the calcarine fissure the cerebral hemisphere has become solid through the increase of the white substance. If the stem, therefore, does not belong to the parieto-occipital, this fissure forms no permanent bulging into the ventricular cavity.

Upon phylogenetic grounds, we might infer that the stem belongs solely to the calcarine fissure. In the chimpanzee the parieto-occipital does not run into it, but is separated from it by a thick superficial convolution. In many human brains this convolution is represented by a deep bridging gyrus, which acts as a feeble barrier to the free communication between the two fissures. In the orang the condition may be exactly the same as in the chimpanzee, or resemble that present in man. In an orang's brain in my possession the right hemisphere shows a free communication between the parieto-occipital and calcarine fissures, whilst on the opposite side the two fissures are completely separated by a superficial convolution.

But on studying the ontogenetic evolution of the two fissures, it becomes apparent that in the human brain the stem of the ≻ may have a different connection in different specimens. In cases where both fissures persist from the precursory stage the stem is common to both. It is the lineal descendant of the broad shallow gutter, which we have noticed in some brains at the fifth month, leading forwards to the hippocampal fissure. It is formed by a depression

[a] "Ueber die typische anordnung der Furchen und Windungen, &c. Archiv für Anthrop. 1869. P. 232.

of that portion of the primitive gyrus fornicatus, which lies between the two anterior extremities of the precursory fissures. In other cases it is formed by that fissure, which persists after the obliteration of its fellow. Sometimes, therefore, the stem belongs to the parieto-occipital, sometimes to the calcarine, whilst there are still other instances in which it is common to both.

This affords an explanation as to the very different degrees of development we continually observe in the calcar avis in the adult human brain. When the calcar avis is small and feebly marked, it corresponds only with the back part of the stem of the ≺, and in this case it is probable that the parieto-occipital fissure is solely responsible for its production. When, on the other hand, it is a strongly marked prominence it will be seen that it is related not only to the stem of the ≺, but also to a portion of its calcarine branch.

In the chimpanzee the calcar avis is very strongly marked, and produced from end to end by the calcarine fissure. Close to the place where the posterior horn joins the body of the ventricle another elevation, placed above the calcar avis, is sometimes to be remarked, and this seems to correspond with the lower end of the parieto-occipital fissure. It is right to add, however, that although I have made sections of two hemispheres from different brains, with the view of clearing up this latter point, I cannot say that I am absolutely satisfied upon it.

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Printed by JOHN FALCONER, 53, Upper Sackville-street, Dublin.

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